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A Method for the Determination of Corrosion Rates by A.C. Polarisation Resistance Measurement

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SYNOPSIS

An experimental technique has been developed for the rapid determination of corrosion rates using the well established Stern-Geary equation. The value of the technique was demonstrated for the corrosion rates of aluminium brass in sea water, and sea water containing 0·01% sodium dimethyl dithiocarbamate for which the corrosion characteristics had been established previously. An instrument based on these principles could be developed for a variety of corrosion measurement applications.

Introduction

The measurement of polarisation resistance has been used for the determination of changes in corrosion rate,[(1), (2)]. The theoretical basis on which these measurements depend was deduced by Stern and Geary [(3)]. The relationship between polarization resistance and corrosion rate is given by the equation:—

$$\left.\frac{\Delta E}{\Delta I}\right|_{\Delta E \to O} = \frac{\beta a \ \beta c}{2{\cdot}3 \ I \ corr \ (\beta a + \beta c)}$$

where Δ I is the applied current required to shift the corrosion potential by a small amount Δ E.

I corr = corrosion current

β a = Tafel slope of anodic polarisation curve

β c = Tafel slope of cathodic polarisation curve

It is generally agreed that the potential shift must not exceed 10 mV but preferably should not be greater than 5 mV and may be either positive or negative. Thus if conditions are such that the Tafel slopes of the anodic and cathodic curves are constant, changes in the corrosion current (I corr) may be determined from the measurements of polarisation resistance. The experimental difficulties arise in the measurement of small polarising potentials, and the potential/current relationship being time dependent. A method of overcoming these difficulties which seemed worthy of investigation was to apply a periodic polarising current from a square wave generator.

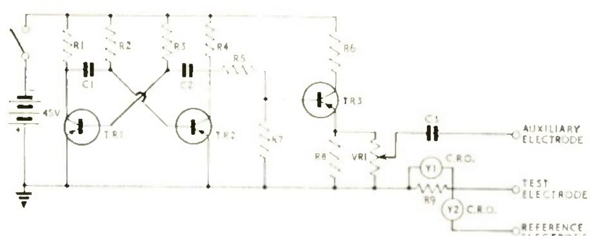

FIG. 1. Square wave generator and electrode assembly for A.C. measurement of polarisation resistance components:—

R_1 & R_4—1K	R_9 —1K High stability
R_2 & R_3—56K	VR1 —2·5K linear
R_5 —3·9K	C_1 & C_2—0·5μF
R_6 & R_7—10K	C_3 —1000μF
R_8 —330Ω	TR_1 TR_2 & TR_3—CV7087 (GET 875)

Experimental

The square wave generator consisted of a transistorised astable multivibrator circuit with an emitter follower as shown in Fig. 1. This multivibrator had a frequency of 25 c/sec. In order to avoid d.c. coupling of the test electrode to the auxiliary electrode, a capacitor (C_3) is placed in the circuit. The potential shift was measured between the test specimen and a saturated calomel reference electrode with an oscilloscope having an input impedance of 1 megohn. The same double beam oscilloscope was used to determine the polarising current by measuring the potential drop across the 1000 ohm resistor (R.9). In the absence of the electrode assembly, with the auxiliary and

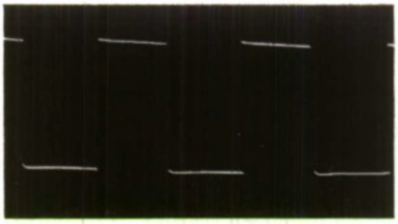

FIG. 2. Waveform from multivibrator.

test terminals shorted, the wave form of the signal generator measured with the oscilloscope across R. 9, was reasonably square as shown in Fig. 2.

In order to test the instrumentation an assembly comprising a saturated calomel reference electrode, a large platinum gauze auxiliary electrode, and a 1 sq. cm. aluminium brass electrode as the test pieces were immersed in sea water. The variable potentiometer was adjusted to give a deflection on the oscilloscope equivalent to 5 mV anodic polarisation, and the current flowing was measured on the second trace as shown in Fig. 3. For comparative purposes this same measurement was taken

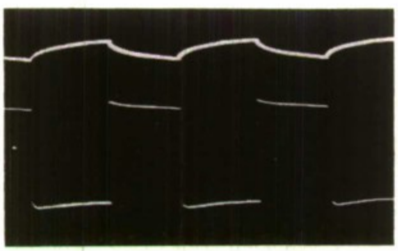

FIG. 3. Upper trace potential and lower trace current for measurement of polarisation resistance on aluminium brass in seawater.

with the sea water containing 0·01% sodium dimethyl dithiocarbamate (SDD) which is known to act as a corrosion inhibitor for aluminium brass[4] with the result that the polarising current was considerably reduced as shown in Fig. 4.

The actual values for the polarisation resistance calculated for the curves shown in Fig. 3 and

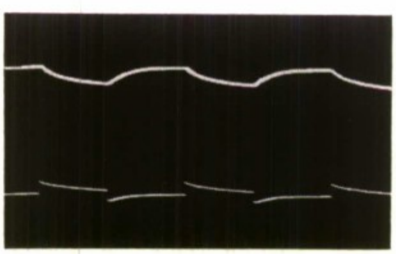

FIG. 4. Traces as in Fig. 3. for aluminium brass in seawater containing 0·01% sodium dimethyldithiocarbamate.

Fig. 4 for polarising currents at the mid positions of the wave are as follows:

Aluminium brass in sea water

$$\frac{\Delta E}{\Delta I} = \frac{5 \text{ mV}}{0{\cdot}054 \text{ ma}}$$

Aluminium brass in sea water +0·01% SDD

$$\frac{\Delta E}{\Delta I} = \frac{5 \text{ mV}}{0{\cdot}003 \text{ ma}}$$

From previous work[4] it was known that the addition of the inhibitor SDD had a negligible effect on the slope of the anodic (β a) and cathodic (β c) polarisation curves, the inhibition being due to a shift in the position of the anodic curve. The polarisation resistance may therefore be taken as inversely proportional to the corrosion current.

$$\frac{\Delta E}{\Delta I} \propto \frac{1}{I \text{ corr}}$$

Thus the corrosion rate of aluminium brass was reduced due to the addition of inhibitor by a factor of $\frac{0{\cdot}003}{0{\cdot}054}$ giving a value of 0·056.

To obtain the Tafel slopes β a and β c for substitution in the Stern-Geary equation the polarisation curves for aluminium brass were determined potentiostatically in still sea water at 20°C. Stepwise adjustment of the potential was made at 20 mV intervals every 10 minutes using a potentiostat. Potentials were measured with reference to a saturated calomel half cell, and platinum gauze was used as the auxiliary electrode. The polarisation curves obtained are shown in Fig. 5, from which the slopes β a and β c were measured and the corrosion current extrapolated as 9·4μA/sq.cm. Substituting the values of 0·045 and 0·60 volts/tenfold increase in current for β a and β c respectively gave a value for the expression $\frac{\beta a \; \beta c}{(\beta a + \beta c)\; 2{\cdot}3}$

Using the measured values of β a and β c the calculated corrosion current from the Stern-Geary equation was approximately 200 μA/sq.cm. which was much higher than that extrapolated from the polarisation curves given in Fig. 5. The likely explanation for this discrepancy is that polarisation resistance measurements were made on a freshly prepared specimen for which the corrosion rate would probably fall with time. This explanation was confirmed by exposing similar specimens of aluminium brass in sea water and determining the polarisation resistance at various time intervals. Fig. 6 shows how the corrosion current for aluminium brass fell with time to a value similar to that predicted from extrapolation of the polarisation curves shown in Fig. 5.

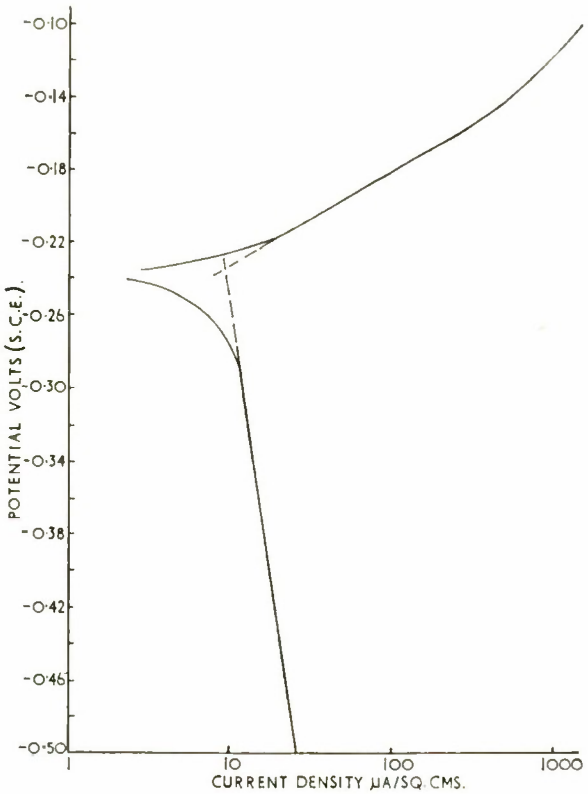

FIG. 5. Polarisation curves for aluminium brass in seawater at 20°C.

Discussion

The polarisation resistance measurements have been applied to a corrosion inhibitor system for which the corrosion characteristics have previously been investigated in some detail in order that the square wave polarisation resistance technique could be evaluated. The effect of addition of the inhibitor SDD on the corrosion of aluminium brass was found to be reasonably consistent with previous measurement, and demonstrates the capabilities of the instrument. In Figs. 3 and 4 it is evident that the potential curve is rising and the current decreasing during the square wave cycle; this difficulty could be overcome by using a smaller potential shift conforming to the requirement of the Stern-Geary equation that ΔE must be small, or by reducing the frequency of the oscillator. The frequency of the oscillator has since been reduced to 5 c/s by increasing C_1 and C_2 to 2·5μF. It has been observed that by applying the A.C. current to the system there was a small shift in the D.C. potential of the test electrode. If this condition should produce departure from the Stern-Geary conditions it could be overcome by using a smaller A.C. potential shift.

The precise shape of the wave form from the oscillator (Fig. 2) was determined by the capacitor C_3 a high value being required for the output from potentiometer VR1 to be square. In Figs. 3 and 4 the peaks in the current curves were assumed to be due to the capacitance of the Helmholtz double layer at the surface of the test electrode.

The use of the Stern-Geary equation is well established for the comparison of corrosion rates. The use of a square wave generator in conjunction with an oscilloscope has been demonstrated to be a simple technique for the measurement of polarisation resistance. The instrument was evaluated using an inhibitor system, but the technique was developed primarily to study the effect of flow rate on the impingement attack at the inlets of heat exchanger tubes which is presently being investigated. It appears that the instrument has potentialities for a variety of applications where instantaneous corrosion rates are required to be determined.

D.C. polarisation resistance measurements have been used for a wide variety of corrosion systems such as mild steel in lithium bromide solutions[5], nickel in hydrochloric acid[6], aluminium in fruit juice[7], a study of corrosion inhibitors[8] and a study of cavitation erosion[9].

There are several factors which may limit the applications of this technique. The electrode assembly must be immersed in an electrolyte of low resistance. It is assumed that during polarisation of the electrode the corrosion is the primary oxidation reaction and occurs homogeneously over the electrode surface. Accelerated forms of corrosion such as pitting and stress corrosion are said to be indicated by erratic results but can not be quantitatively measured.

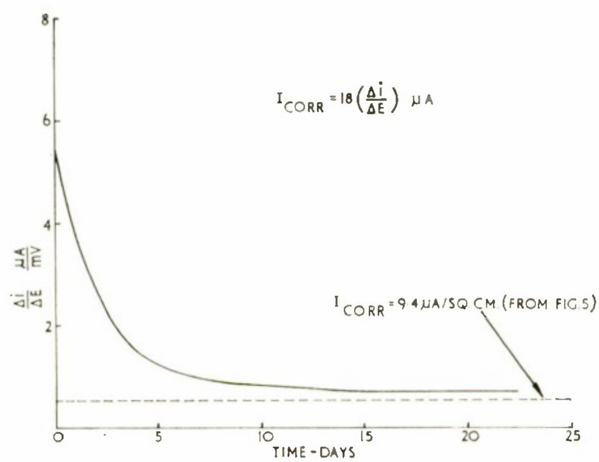

FIG. 6. Change of corrosion current for aluminium brass with respect to time.

It may be envisaged that a portable instrument avoiding the use of an oscilloscope would be useful; this could probably be attained using a transistorised linear amplifier to obtain currents capable of being measured on robust commercially available meters. If test specimens of larger size are required the current output of the square wave generator could be increased using a further emitter follower such as an OC 28.

Whilst this technique has been developed as a laboratory tool for marine corrosion studies there may also be commercial applications as a corrosion meter for use in the chemical and oil refining industries.

Acknowledgements

For advice and encouragement the authors are indebted to Dr. C. D. Lawrence, Mr. J. N. Bradley and Dr. R. Holland. Thanks are also due to Mr. K. C. B. Thomas and Mr. A. W. Hackney for the design and construction of the square wave generator.

The use of this technique for the measurement of polarisation resistance is covered by British Patent Application No. 34157/66.

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OPERATION UP-KEEP AND HIGHBALL

Memories of Wartime Trials on the "Bouncing Bomb"

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Rendezvous

"I must warn you that the security grading for these trials is MOST SECRET. You will discuss the project with no-one except the other members of the trials party."

This was my introduction to a series of interesting and exciting trials during the period late 1942 to mid-1943. It was spoken by Prof. (now Sir John) Carroll in his office at the Admiralty.

I had already been seconded on loan from A.R.L., Teddington on a previous occasion for trials on depth charges in the North West Approaches. This was the second time that he had requested Superintendent A.R.L. Teddington for my services; I took this to mean that he was satisfied with my work on that occasion.

Col. A. V. Kerrison (then Superintendent, A.R.L.) was glad to loan me, but my Group Leader was "hopping mad." However, he had no choice in the matter and so, a few days later, I was waiting on Euston Station to make an evening rendezvous with a Dr. Alan Baxter and a Mr. Robert Price. A four-berth sleeping compartment had been reserved for us and, once the train had started, we sat with heads close together while Alan explained, in a quiet matter-of-fact way, the details of the project.

Maritime Menace

At that time, the greatest danger to our vital shipping in the Atlantic was the German pocket battleship *Tirpitz*, skulking in a Norwegian fjord and awaiting a suitable opportunity for a lightning marauding expedition against our merchant fleet. Her secluded anchorage was well-known to the R.A.F. photo-reconnaissance boys, who were over the fjord almost daily to record any change in her position. Naturally, a ship of her size and importance was well screened with steel-mesh nets to foil any torpedo attack. The problem was to penetrate this screen and press-home an attack which would sink her or at least immobilise her. As her decks were armour plated as well as her sides, a torpedo explosion close to the unprotected keel appeared to be the only method of attack likely to succeed.

Ducks and Drakes

Mr. Barnes Wallis, aeronautical scientist from Vickers, attached at that time to the Admiralty's Miscellaneous Weapons Dept., had proposed a weapon which would bounce on the surface of the sea in a "ducks and drakes" manner. (I believe that some trials had already taken place in the Portland area, but it was felt that the security aspect was unsatisfactory due to the proximity of German-occupied France).

Trials Target

In order to continue the trials, a Scottish loch (Loch Striven off the Firth of Clyde) which closely simulated the Norwegian fjord, had been selected as the trials venue. The former French battleship *Courbet* (22,000 tons) had been allocated to the project as a target ship. She was little more than a hulk and had to be towed into position, near the loch-side in Lock Striven close to the village of Inverchaolain, as her main machinery was out of action.

However, as a target she was ideal, being very similar in size to the *Tirpitz*. Her length was 541 feet, her beam 92 feet 6 inches and her draught about 32 feet. She had roughly 11 in. thick armour which extended 5 feet 6 in. below and 7 feet 9 in. above the water line. She was anchored in a position, relative to the loch-side hills, similar to that of the *Tirpitz* in her fjord. Alan told us that the missiles or bombs, referred to as "stores" for secrecy, would be dropped by *Mosquito* aircraft and that the trials aircraft crews were commanded by Wing Cdr. Guy Gibson, D.S.O., D.F.C., and based at Prestwick Airfield.

Flight Perils

The drill worked-out for dropping the "stores" entailed very difficult and very dangerous flying. It was essential for the aircraft to be flying very fast in order not to be hit by the "store" when it first bounced. The number of bounces and their probable heights and lengths had to be calculated in order to make certain of hitting the armour on the ship's side. This involved bringing the aircraft down to only 50 feet above the sea before dropping the "store" as, otherwise, it would probably miss the armour and pass right through the ship's superstructure. To dive an aircraft having only a wooden fuselage from 10,000 feet to 50 feet, reaching a final speed of more than 400 miles per hour, into a loch entrance less than a mile wide and only straight for 5 miles, with hills on either side almost 2,000 feet high, needed the nerve of a super-man like Guy Gibson. None of the other pilots could achieve the same results as he, in the early days of the trials. More of this later.

Assignment

Our part in the trials was to track the path of the missile after it hit the ship's armour. To achieve a devastating explosive effect on the *Tirpitz*, it would be necessary to keep the missile as close to the keel as possible.

Mr. Wallis's scheme to achieve this close proximity entailed spinning the "store" on a horizontal axis—at 90° to the direction of flight and having a rotation such that the lower side of the "store" was moving in the same direction as the aircraft. This would have three effects when the "store" hit the water:—

a. It would reduce the forward speed of the "store."
b. It would increase the height, and consequently the length, of the bounces.
c. On total deceleration of the "store's" forward speed when it hit the ship, the residual spinning moment would tend to keep the "store" in contact with the ship and roll it round under the keel.

By incorporating in the "store" a train of small explosive charges timed to detonate at specified intervals, it would be possible to track the path of the missile under the ship. The code name for the whole project was *Up-keep and Highball*.

We talked well into the small hours, hardly aware of how the time passed. When I finally "turned-in," my mind was too full of the project for me to get to sleep quickly. I was still wrestling with a problem of how we were going to drive a 12-coach "store" down a 1 in 18 incline at 400 miles per hour into a tunnel 50 feet wide full of sea water, when I was woken up by the train jerking to a standstill in Glasgow Central Station.

All Modern Conveniences

After a maddeningly slow journey from Greenock Boom Defence Depot by MFV (maximum speed 4 knots), we arrived at the trials ship, moored in Loch Striven to find that all our apparatus, despatched previously by Alan, had arrived safely. Some of this was from Mine Design Department, and some on loan from A.R.L., Teddington.

The ship was in the charge of a naval commander who had as crew a C.P.O. and about ten ratings. The only electrical power supply available in the ship was DC at 110V, provided by a dynamo, belt-driven by a steam traction engine, which was lashed with ropes on deck at the waist of the ship on the port side; there was a duplicate system on the starboard side as a stand-by. The few kilowatts available were for emergency lighting only throughout the ship and for cooking in the galley. Fortunately all our equipment was operated by batteries which could be charged occasionally

from the emergency supply. The only heat in the ship was in the wardroom and in the four occupied cabins and was provided by very inadequate oil stoves.

The commander had been requested to provide for us six wooden booms, 30 feet long and to lash these at specified positions, three on the port and three on the starboard side, projecting at 90° to the ship's sides at deck level. These were equipped with ropes and pulleys to suspend the hydrophones and lower them into the water.

The Beauty of Simplicity

In these days of sophisticated scientific apparatus, the equipment which we used then seems crude, but it achieved the desired results and there was little to go wrong. The recording system is described below.

The hydrophones (or transducers as they would be called today) were tourmaline piezo type and we called them "gauges." They had a fairly high impedance and the cables used with them were low impedance co-axials. The six booms carried a total of eight gauges, two forward on port and starboard sides at a depth of 15 feet, four amidships *viz:* two to port at 15 feet and 60 feet repeated on the starboard side, while the remaining two were aft, one on each of port and starboard sides at 15 feet deep. Each transducer was connected by its screened cable to the primary winding of a matching transformer (located below in the recording room) whose low impedance secondary winding was connected to one channel of an Eindhoven String Galvanometer. This galvanometer had, I think, 12 phosphor-bronze stretched strings, 0·001 in. dia. illuminated via a cylindrical lens by a 110V DC *Point-o-lite* lamp. The illuminated slit was imaged by a lens on to 35 mm negative ciné film in a camera attached to the galvanometer. This film was driven through the camera continuously at a constant speed of 10 feet per second by a governed motor running off 110 VDC supplied by batteries.

The camera held 200 feet of film which gave us 20 seconds of running time. No take-up spool was provided and the film simply spewed out of the camera into a cardboard carton of suitable size. This meant that all the recording had to be done in a fully darkened room and all switches had to be located by touch. Our main problem was stray light from the *Point-o-lite* system and through various holes in the bulkheads. We had to seal up all fine chinks in the lamp-house system and plug up the bulkhead holes with wooden plugs. I remember being a bit shocked at the crudity of this arrangement, but, when all stray light had been eliminated, we had scarcely any trouble. The worst that could, and did, happen was occasional entanglement of adjacent galvanometer strings due to reception of a very large amplitude signal. This sometimes meant re-stringing of one or more of the 12-strings in the galvanometer. As I was accustomed to handling and cleaning fine wires, this presented no problems.

In addition to the eight gauges in the water, there was a further gauge clamped to the ship's side below the water line in the recording room and this recorded the instant of impact. The tenth string in the galvanometer was coupled to a 1,000 c/s electrically-maintained tuning fork driven from 24V DC. This gave us an accurate time scale.

Photographic Practice

Adjacent to the recording room was the dark-room. Our processing equipment for the 35 mm film was crude but effective. We had two tanks (actually hard-rubber submarine battery type) capacity about 20 gallons each, one containing a fast developer, the other an acid-fixer-hardener solution. These two tanks were washed out and filled with fresh solutions for each set of trials, which usually lasted about 14 days.

It took almost a week to set-up and test all our apparatus. We finally checked that the whole system was operational by dropping overboard a $\frac{1}{8}$ ounce cordite charge with a time fuse.

Trials Procedure

The recording drill was simple. Alan was stationed 60 feet above the water line on the three-storey conning tower of the ship. I was with Robert in the recording room below. As soon as the R.A.F. announced over the radio "Ready for Run No. ...", the recording room light was switched off and the *Point-o-lite* lamp was switched on. On the first bounce of the "store", Alan announced "Splash" over our sound-powered intercom. This was the signal to Robert to switch on the Einthoven galvanometer electro-magnet and gave time for the current to stabilise. On the last bounce, Alan ordered "Run." I switched on the camera motor. At the conclusion of the run, the end of the film ran out of the camera into the carton. I then took it into the dark-room and fed it by hand into the developer. After the appropriate development time, I fed it by hand into the fixer. While I was processing the film, Robert would switch off the galvanometer magnet and re-load the camera ready for the next run. When the film had been in the fixer long enough for fixing to be complete, the dark room white light was switched on and the film could be inspected visually, The "fire-cracker" in the "store" produced 10 detonations at intervals of half a second. Thus, the actual recording only occupied about 50 feet of film. This was cut out

and saved while all the remainder was discarded. The 50 foot record was subsequently washed in an adjacent bathroom and hung up to dry. Any intervals between runs were occupied with inspection of the "washing" in the bathroom and spooling and numbering the records.

The Spider's Web

In order to arrive at approximate results quickly, Alan Baxter had devised a very ingenious gadget appropriately named *The Spider's Web.* On the floor of an adjoining room, he laid out a scale plan of the hull of the ship viewed from beneath the keel. The floor represented the surface of the water. The hydrophones were small screw-eyes on the ends of short wooden rods of appropriate length, representing hydrophone depths. At each screw-eye, a long length of Admiralty-pattern trout line was knotted. All the lines were taken up through a large screw-eye in the ceiling. Each line was provided with a small lead weight to keep it taut. Small sliding markers were set on each line at a point representing to scale the time interval between impact and arrival of shock at hydrophone. All the lines were threaded through a ring which was hand-held. The idea was to move the ring around until all the marks on the lines were located in the ring. This then gave the position of the detonation in question. By this means, it was possible to know approximately by 2100 hours the path of the "store" for every run recorded during the day. More accurate results were calculated from the records subsequently.

Security Precautions

All our apparatus was operational in time for the preliminary trial runs without actual "drops." When the "live" trials were imminent, various high ranking officers, Naval, R.A.F. and civilian, including Prof. Carroll and Mr. Barnes Wallis, visited the ship on a day-to-day basis. In order to maintain secrecy, all the service officers wore mufti in order not to excite interest. They came by alternative routes at different times. Some stayed at Greenock, some at Rothesay while others flew over from Helensburgh each day in a Sunderland flying boat on loan from R.A.F. Coastal Command.

Rothesay is a popular holiday resort and, even in war-time, was often full of visitors. It lies due south of Loch Striven and, in fine weather, the whole of the trials area was visible from Rothesay front. To shield the trials from prying eyes, a drifter steamed back and forth laying a black smoke screen across the mouth of the loch during the time of day that the trials were in progress. The shepherds on the loch-side hills were politely, but firmly, requested to move their flocks over to the other side of the hills. During actual trials running time, despatch riders on motor cycles patrolled the rough tracks along the top of the hills to prevent any unauthorised person from viewing the trials. The inhabitants of the village of Inverchaolain were security checked and warned not to discuss the trials among themselves or with outsiders at any time. Living in a remote spot, they were, for the most part, the dour type of Scot ideal from the secrecy point of view. Very careful and unobtrusive security checks were made from time to time both in the village and in Rothesay, but, as far as could be ascertained, the secret never leaked out. No changes in the screening arrangements for the *Tirpitz* were ever reported, so it is reasonable to suppose that the weapon remained unknown to the enemy.

Perils Aloft and Below

The first few runs were made by Wing Cdr. Guy Gibson and observed, from the air and from the ship, by other R.A.F. pilots. These others then tried to emulate his skill with very variable degrees of success. On two occasions Alan Baxter nearly lost his life owing to their lack of skill. The first time was when a pilot dropped his "store" too late and from too great a height (about 150 feet). The weapon only bounced three times instead of the usual four and cleared the top of the conning tower by just a few feet. Considering that the "store" although only 3 ft. in diameter, weighed about a ton and was travelling at perhaps 100 m.p.h., the effect if it had struck the conning tower, may well be imagined.

The second occasion occurred in a later trials period. Soon after arriving at the ship and while we were still preparing for the new trials, Alan developed influenza and we persuaded him to stay in bed. The trials were slightly delayed to allow him to recover but, after several days, he was still not fit. We then decided to start the trials and manage with just Robert and myself. We took it in turns to be up aloft and down below. It slowed things down because the developing and fixing of the film for one run had to be completed before any preparations for the next run could be made. The first run was quite successful, but the second was a "bad drop." The "store" bounced four times, each one too high, missed the armour plating and ploughed its way almost right through the ship, choosing the cabin next to Alan's for its entry. It tore a 6 ft. diameter hole in the steel hull plating, cut its way through two internal bulkheads and was finally blocked by a massive cast iron circuit-breaker cabinet, shock mounted on springs. The noise of the impact and the protesting twang of those springs, stretched far beyond their elastic limit, did nothing to improve Alan's splitting headache. Next day, claiming that he was

much better, he joined me in the safety of the recording room and lent a helping hand there.

Assorted " Gremlins "

Three main troubles be-devilled the early trials. First, the " stores " were not strong enough. Several shattered on first hitting the water. Secondly, it was obvious that coming down to 50 ft. was too nerve-racking for some of the pilots. Thirdly, the inertia switch in the " store " for detonating the fire-cracker was very prone to trigger on the first bounce, rather than on impact with the ship's armour. When the fire-cracker detonated on impact, we were able to plot the missile's path very well. If the inertia switch operated too soon, the run was wasted. There was also a fault in the bomb-release mechanism which gave trouble. As the " store " was spinning on its axis in a gymbal system, there was a bearing for it on either side. These two bearings had to be retracted exactly simultaneously to drop the bomb; retraction of either before the other, caused a violent " yawing " off-course of the store when it fell, due to gyro precession of the rotating sphere. In one instance, one bearing retracted but not the other, causing the " store " to hang half-in and half-out of the plane, still spinning. The gyroscopic effect did alarming things to the steering of the aircraft. The pilot was instructed by radio to " waggle about a bit " over the hills in an attempt to jerk it free. He finally succeeded in doing so and the rest of that day was spent by a party of ratings, hastily brought over from Rothesay, searching the hillside for the lost secret missile.

The trials were temporarily suspended while efforts were made to overcome these problems.

Abandon Ship

By this time, my group leader at A.R.L., Teddington, was resigned to my periodical absences. After the first trials period, he asked me what I had been working on but I refused to say, according to my instructions from Professor Carroll. This caused relations between us to be a trifle strained. However, Col. A. V. Kerrison assured me that, as there was no need for him or my group leader to know details of the trials, I was quite right in not divulging them.

I had only been back on my usual work at Teddington about a week, when I was ordered to report to the ship again. On arrival, I found feverish activity. Instructions had been received from Admiralty that all our apparatus was to be stripped out of the ship immediately, as she was needed as part of *Mulberry Harbour*. In two days, all our equipment was out of the ship and temporarily stored at Boom Defence Depot at Greenock. We learned, after the invasion of France on " D-Day ", that *Courbet* had been sunk as part of the harbour mole to protect landing craft from the rough seas on the Normandy beaches. We were promised another ship as soon as possible.

Target Par Excellence

A short time elapsed and then we were told to rendezvous aboard H.M.S. *Malaya* at Greenock. This magnificent battleship was in full commission and her Captain was distinctly peeved to have his ship chosen as an Aunt Sally for a lot of boffins. *Malaya* dated from 1915 and was a gift to Britain from the Federated Malay States at a cost of nearly three million pounds. Her length was about 634 feet, her beam 104 feet and her draught about 30 feet. She had a displacement of 31,000 tons and had been partly re-constructed during the period 1925-1933 at a cost of a further one million pounds. Additional improvements had cost about £900,000 in 1934. In spite of all this, she was nearing the end of her useful life, as she was too slow compared with more modern ships.

On arrival at the trials venue, a quick inspection of the ship revealed two serious disadvantages as far as we were concerned. First, her armour, 13 in. thick on the water-line, was only 4 in. - 6 in. thick above the water-line. Secondly, she was fitted with " bulges " as additional protection against torpedo attacks. Accidental damage to an old hulk like *Courbet* was one thing, but damage to a commissioned battleship was quite another thing and definitely not done.

Naval Negotiations

The only way to get thicker armour above the water-line and lift the torpedo bulges out of the water was to put a list on the ship. We suggested 7·5° at first. When the Captain recovered from the shock, he countered with an offer of 2·5°. A compromise of 5° was finally reached, as it gave us about 4 ft. 6in. of lift, which was the minimum that we could tolerate. So every morning from then on, very early, the pumps were working at full pressure to shift water in the ballast tanks (and fuel oil too, I believe) from port to starboard side to produce the required list to the ship. Every evening, as soon as the trials were completed for the day, the ship had to be restored to an even keel. A 5° list may not sound much of an angle, but it was surprising how difficult it was to walk about and work under those conditions.

Once our requirements were made known to all in the ship concerned, no task was too much trouble. All our cables were not only run for us, but largely concealed so that the ship should not look too untidy.

We were made temporary members of the Wardroom and wined and dined like lords compared with the spartan conditions aboard *Courbet*.

Satisfactory Conclusion

Our other problems had been solved. The partially-wooden case of the "store" had been replaced with an all-metal one. The aircraft height difficulty had been overcome by a simple, but ingenious, suggestion by someone at H.Q., not concerned with the trial. A small, but intensely bright spotlight was mounted on each wing-tip, pointing downwards and angled inwards, so that the two spots of light merged at a height of 50 ft. All the pilots used this device with complete confidence. The inertia switch had been completely redesigned (see Appendix) and gave no further trouble. The tolerances on the release mechanism had been made more stringent and this now functioned perfectly. When a successful series of trials had been completed, we steamed back to Greenock.

The Captain and the Chief Officer were shortly leaving the ship and a farewell dinner had been arranged for them by the Wardroom. We three civilians were invited to attend and we enjoyed a ceremonial meal which I am unlikely to forget.

We travelled south in a satisfied frame of mind, wondering what the next move would be. We had not long to wait to find out.

Unexpected Results

During our trials period, a specially-formed R.A.F. Squadron, No. 617, commanded by Wing Cdr. Guy Gibson, D.S.O., D.F.C., had been training in the special techniques required to use the new weapon in Lancaster bombers. Apparently, Wallis had always had the plan that the new weapon would be ideal for attacking any dam which was protected by anti-torpedo nets. Most people have heard of the "Dam Busters". It was the bouncing bomb which was used in that famous attack on the Moehne, Eder and Sorpe Dams with such success on May 16th, 1943.

Although the object of the raid was achieved, Wallis was appalled at the loss of life involved. Out of 19 bombers, eight were shot down with 53 R.A.F. Officers killed. Several attempts had already been made to put the *Tirpitz* out of action. After an attack by British midget submarines, she was out of action for six months. The Fleet Air Arm had hit her too, but she seemd to be unsinkable. Wallis had been developing bigger and better bombs. First came the *Block Buster* weighing 8,000 lbs. and used so successfully against Hamburg, Berlin and other German cities. From this developed the *Tall Boy* deep penetration bomb weighing 12,000 lbs. When dropped from more than 15,000 feet, it achieved supersonic speed before impact and had tail fins angled to rotate it as it fell and give it gyroscopic stability. Finally came his 10-ton *Grand Slam*, the earthquake bomb, which had such shattering effects against the V1 and V2 launching sites.

It was decided to attack the *Tirpitz* once again, with the *Tall Boy* bomb rather than the bouncing bomb, as it was hoped that less loss of life would result. In spite of her defensive smoke screens, they achieved at least one direct hit on her in Alten Fjord on September 15th, 1943. She was later towed to Tromso where her *coup-de-grace* was finally administered on November 12th by the same 617 Squadron with the same *Tall Boy* bombs.

If she had not been sunk by the *Tall Boy* bombs, no doubt the bouncing bomb would have had a further success.

Appendix

Admiralty Mining Establishment in "Operation Highball"

By F. H. Evans (A.U.W.E.)

The official records of *Highball* in this Establishment are scanty. This is to be expected especially in the case of most secret projects developed for special war-time operations when telephone instructions were often received direct from D.U.W., London. The first official written intimation of this project was received under D.T.M. (London)'s reference R.S.90/95A/43 dated 6th March, 1943, in which we were told that we would probably be required to assist in the *Highball* project. In fact our assistance went much further than was at first expected.

There is no doubt that the firing and priming arrangements were designed by the late Mr. H. J. Taylor who retired from this Establishment. This officer stated that this is true and that the first 500 firing mechanisms (known as Pistol Type AA and later at Pistol Mk. 24) were made from his original design, tested in this Establishment at Havant, and delivered to Prestwick for trial runs immediately prior to the actual operation against the dams. The hydrophone test equipment for this and other preliminary trials was also supplied by this Establishment. In addition, small numbers of Pistols were supplied from time to time for experimental dropping trials under the direction of D.Arm.R.D.

Liaison was maintained for the most part by personal contact, and Mr. Taylor frequently visited Mr. B. N. Wallis of Messrs. Vickers-Armstrongs at Weybridge. A number of meetings did take place, however, at which D.S.R. and S.M.D. were represented.

It is understood that the war-time Ministry of Aircraft Production were responsible for the weapon itself (which was designed by Mr. B. N. Wallis) but that the Admiralty advised and designed the priming and firing arrangements and, in fact, actually supplied the firing pistols through D.A.S.

SOME THOUGHTS FOR AN UNDERWATER WEAPON SYSTEM OF THE FUTURE

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Introduction

As an engineer I feel that there is "something rotten in the state of Denmark" when we consider weapon systems.

(a) We don't think system-wise.
(b) We are frightened of innovation and creative design.
(c) We try to work against nature and not with her.
(d) We try to meet all requirements at equal importance instead of "MUSTS and SHOULDS".

At present a class of submarines become obsolescent and in due course obsolete. Therefore by a given date we must have replacements.

A new class is ordered and designed. Since we must progress, the new class must go faster and deeper and possibly further than the old one. First of class will be at sea at a prescribed date.

There may be a new weapon as such under development. Will the submarine design be made to suit the weapon? It is unlikely. The in-service date is sacred. The new weapon might not come to fruition. Hence the submarine must be capable of firing everything produced since the flood.

I do not accept the argument that the submarine can be built quicker than the weapon. If the advance in technology/performance is of the same order as demanded for the weapon, the time scales will not be vastly different.

If the submarine is properly designed as part of the weapon system, the time scales will be integrated. It should not be designed at all until sufficient development work has been done to show that the overall system has a fair chance of succeeding.

Lionel Sefton (Premier Precision Ltd.) once said "Achievement is the spur to merit. New products and superior techniques evolve—they don't erupt! To maintain progress it is necessary to produce and then improve . . . the pattern of effective creative expression applied to new design concepts rarely emerges as complete perfect invention. Technical projects that daily train and develop fundamental experience, which extends practical knowledge to maintain competitive capability, are essential to technical impetus and economic progress."

We must produce our ideas, scale models, etc. and do the evaluation experiments BEFORE the words "in-service date" are spoken, but we must have the creative ideas first.

A recent statement in the technical Press said "Whittle nursed his jet engine as an apprentice and brought it to fruition before the mathematicians could prove it impossible." This is the sort of spur that we need.

Over many thousands of years Nature has evolved some excellent designs. She does not attend LTC meetings to introduce modifications—indeed she proceeds slowly step by step and draws on the experience of millions of samples for system reliability etc.

Due to this long evolution, in nature design precedes conception, the latter serving to prolong the species.

In the engineering field the cycle of change is much more rapid, being typically of the order of 10-30 years. Thus in engineering, conception must precede design.

My conception for the future is a submarine weapon system which is integrated from the very start. We must think of the submarine as the first stage of a multistage system.

The final attack weapon is the last stage—there may or may not be intermediate stages. The stages must be compatible and jointly designed and progressed.

The prerogative of the creative designer is to be able to stand on his head and see if things look better that way up.

We all know what a submarine looks like—a cigar-shaped hull with a fin on top and propeller at the back. Six holes at the front for the expulsion of tin fish.

Do we have to accept that they will look like this for all time? There was a time when aeroplanes flew tail first—we might try this—at least it would remove the tendency of submarines to run down their torpedoes before they light up to running speed. This applies particularly to the modern wire-guided torpedo which can get its guidance wire caught up in the submarine's propeller(s).

During the First World War someone had the bright idea of firing machine guns through the aeroplane propeller with appropriate synchronisation. One would hardly suggest doing this with torpedoes and submarines — particularly if the torpedoes are wire-guided! However, the airmen soon resorted to putting the guns on the wings with the engine in the middle; or *vice-versa*.

This should set us thinking. Do we put the tubes in the best place, or the propellers!

The submarine is an expensive fighting system. There is little virtue in being able to go fast and/or deep unless other constraints are satisfied. The blunt bow of the modern nuclear submarine gives severe cross flow conditions for emerging torpedoes. There is an old adage about never spitting to windward! If we spit to leeward, *i.e.* fire our torpedoes astern of the moving submarine, we produce much more favourable exit conditions. All modern torpedoes are fitted for angled firing so that course-setting is no problem.

This has another virtue—the bow of the submarine is left clean and clear for the sonar system.

If therefore we assume that the torpedoes can be fired from the stern, let us occupy the very stern end (and return to this later). We have now trodden on holy ground reserved for the propeller. Even engineering is at times subject to trends. At present, single large propellers or low speed pump-jets are " with it ". Both of these need massive low speed machinery or gearboxes handling large amounts of power.

It is unrealistic to ask for the same performance at say 20 metres depth as at several hundred or even a thousand metres.

One would hardly expect a modern airliner to have the same cruising speed at sea level as at 10,000 metres.

A submarine designed to travel deep, fast and quiet is not necessarily the best ship to travel shallow fast and quiet. If we want the maximum performance deep we should be prepared to accept some speed limitation shallow because of noise.

Whatever *is* put into the requirement, the achievement by a given date will be that allowed by the state of technology and by the money and effort put into it. The minimum number of *MUSTS* should be specified and target requirements for other parameters laid down.

Propulsion

The pump-jet is not a panacea. The drag is higher than for conventional propellers, though there is little or no torque reaction on the hull. Cavitation at shallow depths and high speeds will be minimised, but at the price of gear noise. The single large propeller will produce large reaction torques on the hull, but will have the associated gear noise and not be so cavitation resistant as the pump-jet.

Is it in fact more important to run shallow and fast, or deep and fast? If the answer is both, then machinery noise has to be traded for cavitation noise. With submarines going deeper and deeper, is it a good idea to have a large hole in the pressure hull through which sticks the propeller shaft?

Anywhere greater than about 1 metre from the hull of the submarine is outside the boundary layer, and is thus suitable for propellers.

FIG. 1. Arrangement of motor pods.

With improvements in secondary batteries it is more than likely that any future conventional submarines will carry hydrocarbon fuel as their primary energy supply and use electric motors for all propulsion. So we follow the modern aircraft trend and put the motors in pods at the rear of the " fuselage " (see Fig. 1).

The pods need contain only the motor and built-in cooling system with electrical connections through watertight glands into the pressure hull. Since the output power of the electric motor is proportional to its speed, doubling the speed will double the power. By using torque-balanced torpedo-style contra-rotating motors (and hence contra-rotating propellers) we can produce the double relative speed for normal propeller speed. With two pods, they would each be about 4m long × 1·5m dia for conventional submarines and 7m long × 3m dia for nuclears. Pods could be detachable in dry dock for motor overhaul.

Since hydrostatic pressure will try to push the propeller shaft through the pod, why not put the propellers at the front of the pod and trade off propeller pull against the hydrostatic thrust? At a depth of about 400m they will just about balance,

the exact point being chosen on propeller shaft diameter vs speed and operating depth. Double thrust bearings at much lower ratings than normal can then be used.

MAIN MACHINERY

Conventional

(a) This could be diesel electric combined with battery electric as at present or
(b) Gas turbine electric combined with battery electric.

The gas turbine has reached the stage where good long-term reliability can be expected. Driving a high speed alternator followed by either solid state rectification to charge the batteries, or thyristor control direct to the motors, this would give a very compact power plant. (Bristol Siddeley produce a " pre-packed " Proteus 2·7 MW set and 12-80 MW Olympus sets.)

Nuclear

Here the immediate thought is the conventional turbo-alternator with the turbine being on either a recirculating gas cycle or the normal steam/condenser cycle. Again, thyristor control to DC motors could be used. Standby batteries can supply the motors for emergency drive at lower powers.

Possibly more attractive in this case is to rectify the supply and use a variable frequency DC link invertor feeding 3 phase squirrel cage induction motors in the pods. These motors are extremely rugged and reliable, and using slip frequency control can have very low armature resistances and are more efficient than the equivalent DC motor. The main turbo generator can also supply standard ship's mains.

If the turbo generator supplies only the propulsion motors (with separate arrangements for ship's mains) the induction motors can be connected directly to the generator using a multipole configuration to " gear them down " to the required shaft speed. Here the generator would have to be variable speed.

There is also the possibility for both conventionals and nuclears to have hydraulic motors in the pods with the appropriately driven pumps inboard.

The Weapon Pack

As stated earlier we reserved the extreme tail end for the weapons.

A 10 cm dia hole in a submarine at 1,200 metres depth will let water in at about 1,000 Kg/sec. The thought of five or six torpedo size holes with only bow caps and rear doors between the crew and instant oblivion disturbs me more than a little.

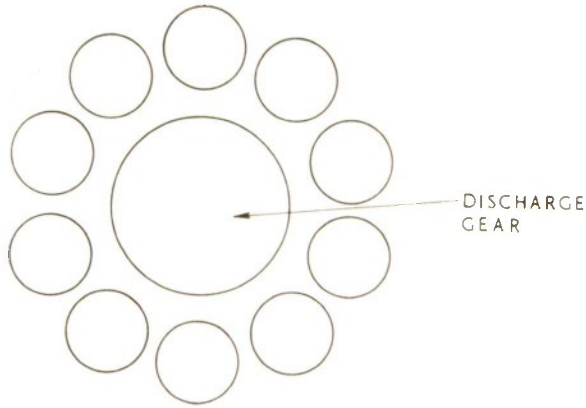

FIG. 2. 10 Tube arrangement.

Why not carry the weapons outboard ? Fig. 2 shows a possible arrangement in which 10 tubes up to 75 cm dia are arranged symmetrically about the axis of the boat. The discharge system is carried in the central space between the tubes. All tubes would be pre-loaded and liquid or gas filled to preserve the torpedo. The " bow cap " would push off with excess internal pressure. The complete pack weighing some 80,000 Kg and displacing say 40,000 Kg would be removable—it would occupy the last 10 metres of the boat. The automatic clamp-on fixing point would be at a bulkhead just behind the rear of the pressure hull.

If a major modification of the weapon takes place during the life of the submarine, only the detachable tail end needs renewing. If we decided to adopt a French torpedo (550 mm dia) we could fit the appropriate firing/tube/weapon package. At present, no matter how good such a French torpedo may be, it won't fit a British submarine.

Discharge System

In the past, partly because of salvo firing requirements, large stored energy discharge systems have been built in. To fire a torpedo against high ambient back pressures requires a great deal of power. Air systems are prone to run-backs if the air bubble is prevented from escaping. Conventional water-ram systems are very inefficient. Why not work with nature instead of against her ?

If a double-ram hydraulic intensifier (Fig. 3) is used, admission of water to cylinder A will drive piston B forward. If both cylinders are about 2·5m long and 1·3 and 1m dia respectively, the weapon will not only have thrust over its complete displacement, but, neglecting losses, at any depth below about 15 metres, the muzzle velocity will exceed 12 metres/sec. All we have to do is prime

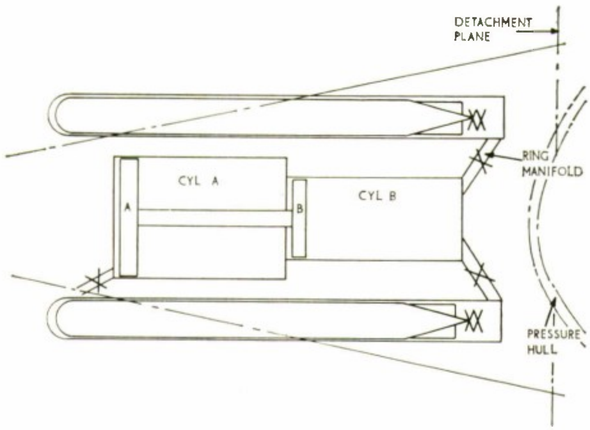

FIG. 3. Double ram discharge system.

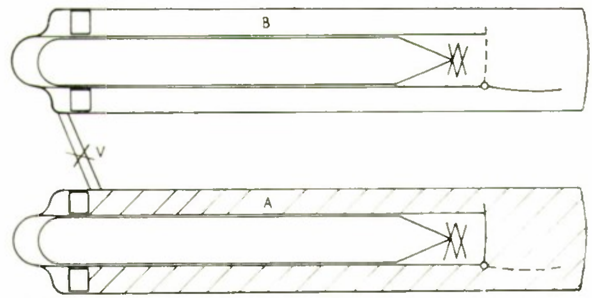

FIG. 4. Alternative discharge system.

cylinder B, select the appropriate valve on the ring manifold and open the valve to cylinder A.

At shallow depths, for higher submarine speeds (if indeed these are desirable at shallow depths) low pressure air could be fed into A to give higher discharge speeds relative to the submarine, and subsequently exhausted into the submarine.

At depths greater than say 80m some form of throttling and cushioning will be needed.

On the basis of non-salvo firing about 1 minute could be allowed for re-cocking.

An alternative discharge system is shown in Figure 4. Each tube is double walled (762 mm and 534 mm) with an annular piston sliding between them.

To fire the torpedo in B, the annulus A is charged with compressed air and B is flooded up. Opening valve V drives the annular piston to the rear thus water ramming the weapon in B.

With tubes A-J arranged in a circle A fires B, B fires C . . . J fires A. In this system there is no water throttling in "small" pipes and valves.

The Sting in the Tail

There is room in the extreme tail to have either

(a) A stern transducer

(b) A towed decoy

(c) Possibly both

The decoy could be deployed from a sealed winch and wound back into its housing as required.

Damage Control

The early balloonist soon realised that tossing sandbags overboard was a useful saver when he lost buoyancy. As submarines go deeper they become more and more vulnerable to rapid flooding—this has already been mentioned above; 40-80,000 Kg of jettisonable ballast would give some 160 seconds at 1,200 metres after being penetrated by a 5 cm dia hole. Not only would it give time, but also very significant lift to take the submarine into shallower depths and reduce the rate of intake.

It seems a pity to carry sheer ballast. If oblivion is the only alternative, anything could be jettisoned. This now brings us back to the weapon pack. In addition to automatic clamp-on, the pack could be explosively jettisoned—this is current technology in space vehicles. It is unfortunate that this is at the wrong end. Whilst to rise backwards is better than not to rise at all, it should not be beyond the wit of man to design part of say the sonar system at the forward end for emergency jettison. The forward part could be jettisoned first and the after part after the boat has started to nose up to the surface.

Conclusion

The above dissertation is not an attempt to design the system, or to tell the other chap how to do his job. It is an attempt to make people think. In a world where it is increasingly difficult to obtain high risk money; where one is frequently not allowed to start a job unless success, on time, and within a forecast financial limit is promised at the outset, it is even more essential that the engineer thinks ahead of Staff Requirements.

His job is to make progress in Technology and not to be diverted from his technical honesty by being prodded by the politician into quoting firm dates on flimsy evidence (Parkinson—The Law and the Profits).

And finally, to those of my heretical colleagues who have not yet changed over to the SI system of units, 534 mm = 21 in.

THE GREAT SMOKE SCREEN

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It is difficult to see a connexion between wedding cake and the problem of atmospheric pollution. The cakes made to a recent B.B.C. recipe suffered from the lack of sugar. In the recipe, the obvious had been overlooked. In the case of pollution there may be some instances where omissions arise from ignorance, but it is difficult to escape from the conclusion that some aspects are discreetly pushed into the background.

In the U.K. responsibility for air hygiene lies with Local Authority. This is only one of the many problems confronting the unfortunate individuals concerned. Other duties leave little time for the study of the copious literature and often policy is coloured by the pronouncements of interested persons purporting to be unbiased experts.

Even with an adequate grasp of the problem and all the best will in the world, it is not easy for local effort to grapple with what is essentially a national problem. A national attack on the problem will result only from widespread public pressure and this is the last thing desired by those who have a vested interest in creating and maintaining pollution.

A great deal is heard and written about Smokeless Zones (often mis-named "Clean Air Zones"). Those who have not had the misfortune to live in an industrial area, cannot visualise the extent to which grimy soot penetrates every facet of family life. The creation of small areas where the filth is less intense is not a matter to boast about. The incredible thing is that people, who consider themselves to be reasonably educated, are content to go on living under such dreadful conditions. Many are not. This is perhaps a major factor in the shift of population to rural areas and in the difficulty experienced in the attempts made to re-populate depleted districts. The progressive despoiling of vegetation and loss of good agricultural land which results is reminiscent of the primitive nomad tribes, who burnt successive tracts of forest land to provide for each crop before moving on and repeating the process. From this point of view the effort to educate people in basic cleanliness, wherever they find themselves, is eminently praiseworthy, but it only touches the fringe of the pollution problem.

Except for a few toxic effluents from particular industries the bulk of pollution arises from combustion of fuel in one form or another. Responsibility therefore rests with those whose interests are bound up with one or other of the highly competitive types of fuel. The elimination of the obvious short term effects due to smoke and soot, leaves the public exposed to the effects of the less obvious but more dangerous constituents. This is a situation where it is easy to induce a sense of false security.

The spokesmen for the various interests concerned are no more dishonest than the Q.C. who employs his skill to present the best possible case for his client. In many pronouncements on the subject, use is made of an age old technique in which there is a deliberate selection and careful arrangement of irrelevant truths, thus diverting attention from other aspects which it is convenient to obscure.

A typical piece of sophistry is to introduce the Californian Smog, and point out that, owing to the absence of the particular natural lighting which brings it about, the U.K. is spared this nuisance. Care is taken however to avoid pointing out that it is the filth in British air which prevents this nuisance. It is this filth which thus deprives man, beast and plant of part of the sun's rays and often necessitates the artificial supply of the radiation lost. Equal care is taken to avoid mentioning that British automobiles also emit the same contaminants into the atmosphere. When (if ever) the smoke and soot is finally eliminated from the atmosphere, enough ultra violet will reach the lower levels to produce a similar (obvious) nuisance in U.K.

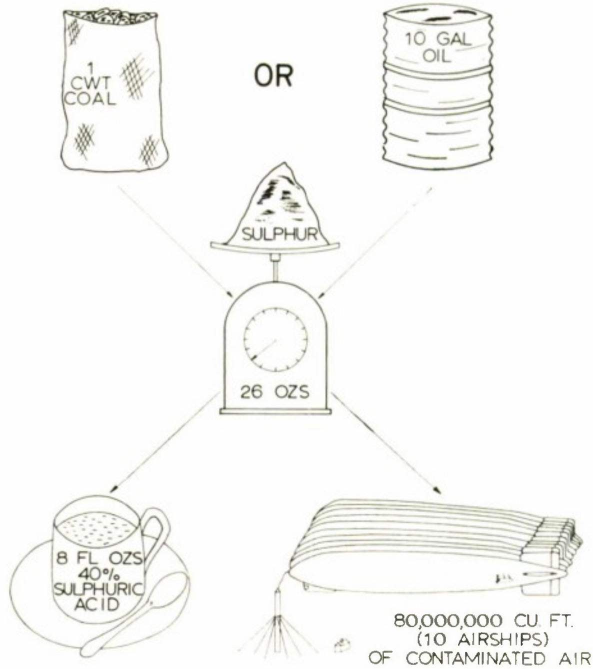
To avoid any chance of these deductions being drawn, the argument may be quickly switched to the special case, which arises from the traffic problems in large cities such as London; namely

the toxic condition due to carbon monoxide. This is very much a U.K. problem since the enhanced concentration arises from idling and deceleration. In California automobiles do not idle and rarely decelerate. Apart from the manufacturers of motor vehicles, no vested interest is disturbed by emphasising the dangers of CO poisoning to pedestrians, traffic wardens and point duty police. The U.S. legislation enforcing an efficient fuel burning system on all imported vehicles may in any case compel all British motors to be so modified in the interests of standardisation. However, while on the subject of street poisoning by motor vehicles, a subject which must never be mentioned is the toxic danger from lead. This is a constituent of the anti-knock dope added to petrol and is emitted continuously while the engine is running. It appears as a colloidal dispersion of particles of a submicron size, which easily penetrates the respiratory system. These minute particles permeate the alveoli and are dissolved at the endothelium, thus passing directly into all parts of the organism. A given amount of lead absorbed in this way is more toxic than an equal amount ingested through the mouth.

The effects of lead poisoning have been known for a long time, but the long term absorption of very small doses has been studied by Dr. S. C. Gilfillan[1] who has traced this as one of the significant factors, contributing to the collapse of culture, progressiveness and genius in the Roman Empire. It is for consideration as to how far this contaminant has contributed to the decline of robust British individualism during the later period of growth of the internal combustion engine.

In a previous contribution[2] an attempt was made to review the general problem of pollution. Attention was drawn especially to the lesser known facts concerning suspended submicron particulates. In this connexion emphasis was laid on sulphur as a major contaminant. It appears as gaseous sulphur dioxide, augmented by a 40 per cent sulphuric acid aerosol. This pollutant arises just as much from the burning of oil as from coal. In fact some crude oils exceed the average sulphur content of coal. Nevertheless there is a noticeable tendency to shy away from any mention of sulphur when pollution is being discussed.

That the problem is of vital importance is shown by the content of a course of lectures on atmospheric pollution given last year at Imperial College[3] These lectures were confined to the consideration of smoke, soot and sulphur. Processes for the recovery of the latter were mentioned, but the bulk of the course was directed toward a different objective. In general the effluent from a stack takes the form of a turbulent cone with a more or less horizontal axis. Given a knowledge of meteorological parameters, an estimate can be made of the position—relative to the stack—where the ground contamination is maximum. Providing the concentration at this point is not above a tolerable limit, information is thereby obtainable regarding the siting of a stack—emitting a known amount of pollution—*vis á vis* human activity.



There are those who contend that it is impractical to remove toxic substances from the products of combustion. They would do well to refer[4] to the situation which resulted in international litigation between Canada and the U.S.A. Wholesale crop destruction due to sulphur oxides occurred in the State of Washington from the pollution emitted by the Consolidated Mining and Smelting Co. of British Columbia. During the six years 1932-1937 the damage caused in the State of Washington was estimated by the U.S. to amount to nearly two million dollars from this one source alone. During the period 1937 to 1940 a recovery plant was installed and operated by the Company. Out of a *daily* output of 800 tons of sulphur oxides, nearly 600 tons were recovered as fertiliser in the form of ammonium sulphate. In the introduction to this paper, Hewson estimated the loss (in 1945) to U.K. from sulphur oxides to be 10 shillings per ton of coal burned.

The real problems of pollution are not technical. They arise from the eternal triangle of unenlightened administration, powerful vested interests and the indifference of an ignorant or at

least a misdirected public. Notwithstanding the known facts of pollution, the oil interests have—despite foreign currency difficulties—succeeded in ousting their competitors not only in traction and industry, but also to a large extent in space heating. The results of the sinister widespread growth of domestic oil fired central heating have yet to be felt. Public concern regarding pollution has been allayed, on the one hand by the emphasis laid on smokeless zones and on the other by the publicity afforded to lung cancer. The successful association of the latter with cigarette smoking on the basis of a statistical hypothesis has became—by constant repetition—to be taken as an unquestioned fact. These two factors have diverted attention from the morbidity due to pollution. The connexion is supported by equally significant statistics together with measurable results of clinical observations.

Unless the effects of pollution are immediate and dramatic it is unlikely that public interest will be aroused. Another disaster similar to the Smog of December 1952 could be decisive, as the rôle played by sulphur oxides is now being recognised by unbiased authorities on the subject. In the meantime reliance must be placed on the long awaited report on pollution by the Royal College of Physicians. It is to be hoped that this report and the resulting propaganda will be on a level at least equal to their previous report on cigarette smoking. This hope is justified since this pending report would be able to rely much more on established fact and less on statistical hypothesis. An example of what might be expected in the way of propaganda posters is included in this article.

If this does succeed in arousing public conscience, the problem would assume an administrative form at a national level.

A national plan of attack on the problem must have two principal objectives in view. In the first place the aim must be to prevent contamination which could affect health or well being. The second consideration would be to recover in a useful form the chemical elements contributing to the contamination. It may well be that the recovery process as a sole objective might not be economic, but it is the height of futility to say that a known toxic condition should be tolerated because its removal and recovery is not profitable. A more realistic attitude is to realise that the recovery of material having any value at all would necessarily reduce the cost of removing the contaminant.

There is of course an advantage in attacking the problem after other countries have worked on it for some time. Today much reliable information is available as regards toxic dosages of various contaminants and the physiological effects from the long continued inhalation of smaller amounts. Further work remains to be done on the synergistic effects of toxic gases in the presence of aerosols. The importance of this effect can be realised by a further reference to the suphur oxides. In America Amdur, Melvin and Drinker showed that exposure of animals to concentrations of 0·35 to 0·5 mgr/M^3 of sulphuric acid aerosol produced rapid pulse respiration rates and lower intake volumes. Such changes were observed after exposure to similar concentrations of 1·62/M^7 mg of SO_2 and 0·12 mg/M^3 of H_2SO_4 (Total 1·73 mg/M^3).

The peak allowable concentration permitted in the U.S.S.R. is 0·5 mgr/M^3 of SO_2 and 0·3 mgr/M^3 of H_2SO_4 (Total 0·8 mgr/M^3). In the U.K. separate determinations of SO_2 and H_2SO_4 are not made.

In the U.K. the average winter concentration of total S (as H_2SO_4) is 0·57 mg/M^3 rising to a normal maximum of 2·86 mg/M^3. In smog conditions the latter is exceeded.

There is another advantage in being a late starter, in that methods of measuring concentration have been studied for many years. These relate not merely to the optimum physical and chemical processes, but also to such factors as the representative duration for taking a sample.

So far as recovery is concerned a 4% sulphur content in oil can be reduced to 1% by hydrogenation under pressure in the presence of a catalyst. The cost of the process is less than a penny a gallon. Where recovery is impossible for one reason or another, additives may be incorporated with a fuel to fix the sulphur as sulphate in the ash. Such methods have been studied by those who wish to protect their expensive stacks from sulphuric acid attack. Unless this acid can be successfully expelled into the surrounding atmosphere, this attack takes place at the acid 'Dew Point'

A national attack on the problem would therefore require the adoption of allowable limits for various contaminants, a system of monitoring and the adoption of powers to enforce offenders to mend their ways. It might well be necessary to permit a reasonable period of time to allow the appropriate actions to be taken, during which time the incentive to reform could be maintained by penal taxation, either on the supplier of fuel or the user or both.

References

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F.I.D. CONFERENCE
Scheveningen, September 1966

Reported by A. H. Holloway, M.Sc., R.N.S.S.
Naval Scientific and Technical Information Centre

The conference followed the usual F.I.D. pattern of a series of meetings, some organisational and some of specialist committees, both of which were nominally 'closed' though interested participants could attend and take part in most of the committee meetings; there was also, in addition to the formal occasions a series of 'programme sessions' at which the officers and leaders of the committees talked about their work, and there was opportunity for discussion: both these were open to all participants, who numbered about 300. There was also a series of meetings of committees dealing with sections of the U.D.C. which were confined to those actively working in the field: the one on transport was held on September 29th and 30th and notes on this are available separately.

Classification Research Committee

(Chairman, Mr. R. Molgard Hansen—Denmark).

A number of statements were made, largely from a national point of view. Thus Levy (France) said that the aim of his group was a co-operative scheme by which the subject field would be split up so far as abstracting was concerned but each would be free to use his own classification scheme while using anybody's abstracts: there was no idea of pushing any one system. Coblans (U.K.) spoke of the Aslib effort to rationalise abstracting in documentation, the initial aim being to secure complete coverage in English and Russian with possible translation but preferably preparation of abstracts in both languages, and Newcombe (U.K.) mentioned that J. Mills had compiled a classification scheme for documentation. Borko (U.S.–A.D.I.) followed Coblans by speaking of the hope that the Russians would cover the whole of the Slav literature while the U.S. and U.K. would cover the remainder, the American output being in machine readable form; the bulletins might well use the same main headings while each used its own finer subject breakdown: Lloyd (F.I.D.) said that F.I.D. could provide a list of about 40 main headings; a paper had been prepared containing a list of new, faceted classification schemes.

It was announced that Mr. te Nuyl had retired from Shell and is to work on a comparison of information retrieval systems to test Cleverdon's (reported) conclusion that Uniterms give better results than any other system including the use of grouping.

It was also announced that a list of about 50 documentation classification terms with definitions, in English and French, is to be drawn up for submission to ISO/TC 46 in 1967; this was in apparent ignorance of the work available in ISO/TC 37 and of several other glossaries and lists of terms.

Training of Documentalists

(Chairman, Prof. Majewski—Poland)

The concept of the continuous development of documentation was elaborated, with the rise of newer modes of communication of information, such as scientific films, which needed to be adapted for retrieval. With such developments went the international provision and exchange of training with experts lecturing in different countries and the provision of textbooks, which is in hand. Most of this looked to the future, but progress is being made in the provision of training in developing countries, such as Tunisia.

The committee has links with others and is planning a meeting in London in April 1967 in connection with the symposium on training. Reports were solicited for publication in the proceedings of the F.I.D. conference in Tokyo in 1967.

Technical Information for Industry

(Chairman, Mr. R. E. McBurney—Canada)

This committee aims at the establishment in each of 80 countries of a centre which would act as a national clearing-house and compile national lists of technical journals for industry and evaluate them. The subject matter is being gradually extended from technical to scientific information and the coverage to the developing countries: thus a regional conference on S.E. Asia is being held in Tokyo in 1967.

A paper by M. Ducas (France) on 'Problems in the communication of technical information to Industry' was presented in which he mentioned the

necessity for the centres to be close to the users, advocated that the personnel should have a technical background with subsequent education in documentation and dwelt on psychological aspects of communication; he mentioned that his own centre had 140 technical personnel and 450 others, 6 documentalists, 2 patent experts and 1 linguist; it uses S.D.I. techniques and is being mechanised. A brisk discussion followed in which British contributors were prominent, including an unanswered question on the criteria for the selection of technical journals and several on psychology, with one on the value of inspirational browsing.

Universal Decimal Classification

(Acting Chairman, Mr. D. Newcombe—U.K.)

Mr. Lloyd gave an account of the C.C.C. work in this direction and of the existing tests — full editions in nine languages with a tenth coming with a number of medium and abridged editions. The Russians are producing about seventeen special subject editions as well as a full edition, while the Americans are to make use of the English medium edition in their project for the application of U.D.C. to computerised retrieval, using a text of about one third of the full edition. There are about 20 active revision committees and about 10 more are planned. Among projects mentioned are those for bringing documentation together under a single main number, with Dutch work on information and communication science and American work on revision of seismology and hydrology as well as a number of instances of U.D.C. mechanisation.

In the discussion there was some criticism of the functioning of revision and publication; a speaker from Tunisia emphasized that the developing countries need adequate schedules and that potential users are being lost for the want of them, Mr. M. Rigby said that full development is needed before commercialisation could bring wide application in the United States and from England it was noted that the slow advance of revision work towards becoming official is a great handicap to present users as well as to potential future ones.

Theoretical Basis of Information

(Chairman, Prof. Mikhailov—U.S.S.R.)

The committee's programme is set out in a paper distributed at the meeting, but this was said to be incomplete; the committee was set up in 1965 and it appeared still to be collecting ideas, though there were indications of an authoritative attitude to organisation. The discussion was recorded.

The field of the committee's work impinges on those of a number of other committees and the difficult task of defining it was emphasized; it was suggested that funds should be acquired for setting up research projects, the organisation of symposia and the production of state-of-the-art reviews. It was asserted that the use of information on documents is steadily decreasing and that they need to be overhauled in the interests of users. Other points mentioned were that the fields of theory, experiment and information overlapped, the necessity of co-ordination of work with other groups, the importance of terminology and its development, and the usefulness of the exchange of thesauri. One interesting point raised by the chairman was that the whole of the field cannot be covered under the aegis of V.I.N.I.T.I. and that there is a necessity for international collaboration.

Classification Research

(Chairman, Mr. R. Molgard Hansen—Denmark).

This committee has developed terms of reference and has produced a report on five years of work. It maintains liaison with classification research groups in a number of countries, and the session took the form of a number of reports on work in progress in a number of places. Among those mentioned which did not arise in the committee meeting are the compilation of a report by M. de Grolier on his experiences in the U.S., a U.S. scheme for compiling guidelines for the construction of thesauri (mentioned by E. B. Jackson as arising out of Project LEX), work at Marseilles on the documentation of documentation and compilation of equivalence lists of terms connected with classification, of which a first draft (of about 500 terms) has been produced, as a basis for the rationalisation of subject analysis: the final draft is expected to contain about 200 terms. Other collections include an archive on magnetic tape of papers in about 150 German biological journals, a collection prepared by I.B.M. of about 150,000 words used in biology arranged according to a faceted classification and a Norwegian polytechnic thesaurus of about 20,000 terms.

Linguistics

(Chairman, Prof. W. H. Locke—U.S.)

F.I.D. work on linguistics, lexicography and terminology was surveyed and a reorganisation of the committee structure announced. The new committee hopes to avoid clashing with other committees and has its terms of reference: a definition of linguistics was read and this sounded rather odd and very comprehensive. There was some emphasis on the necessity of internation collaboration and the usefulness of compiling multilingual equivalence lists in as many fields as possible, though Dr. W. E. Batten pointed out that there are no true equivalents and no constancy in the

use of terms. A Roumanian proposal to include abbreviations in U.D.C. schedules was talked out and mention was made of an approaching publication in Germany of a number of terms in documentation in German, English and French, which has been compiled over the last five years.

Speakers in the discussion seemed in general to be poorly informed about work being done by others, which underlines both the need for international collaboration and its comparative absence at present.

Operational Machine Techniques and Systems (O.M.)

This is a new committee: the session was chaired by Mr. H. Reul (Germany) but Dr. K. Samuelson (Sweden) is to be the chairman of the committee, and he gave a short account of the work of the committee on the theory of machine techniques and systems, of which he is secretary.

The intended activities of O.M. are the study of operational systems including the presentation of reports on them and the estimation of their usefulness in libraries, the stimulation of research and the preparation of the programme for the conference on mechanised information storage, retrieval and dissemination in Rome in June 1967.

There was a short presentation on the Thompson-Ramo-Wooldridge Generalised Information Management System for information storage and retrieval, which from the description appeared to be a very elementary one. Another presentation was on the I.B.M.-I.T.I.R.C. system in which coded abstracts are stored on magnetic tape to be retrieved by the words used in them: the system reads at 120,000 words per minute, search is sequential but from 20 to 300 searches can be conducted simultaneously: descriptors are allocated but are used for S.D.I. only: the tendency in development is towards larger machines and linkages. A new German system was also described in which paper tape is used to produce magnetic tape which in turn gives weekly issues of bulletins with monthly, quarterly and six-monthly cumulations and indices. Separate issues are produced for unpublished reports and for maps; the original six channel paper tape is used to control a Linotype machine but future development should be to printing by photographic reproduction from a C.R.T. display, for greater speed.

General

The programme sessions were excellent in concept but varied in their actual value. With chairmen coming from many countries but with the proceedings being almost entirely in English, the language difficulty was obvious, and indeed one chairman had no English and conducted proceedings through a deputy. The quality of chairmanship varied very widely, some sessions having no noticeable form while others adhered to a rigid programme and in one or two discussion was negligible through lack of time or lack of encouragement. One or two contributors were allowed to read long prepared statements which were sometimes followed by translated summaries, but on the whole the conference was successful, particularly in contrast to the old F.I.D. pattern in which nearly all the sessions were nominally closed and the more interesting ones seemed to be held simultaneously.

The conference was attended by some interesting social occasions, quite apart from the invaluable opportunities for meeting and discussion with other members from a number of countries outside the actual sessions. The first social occasion was a visit to Modurodam, with refreshments, presented by N.I.D.E.R., in which the considerable attractions of the miniature city, port and transport system were not quite equal to those of discussion; the second was a subscribed coach tour and dinner, the tour part of which took 2½ hours to get about 50 miles through not very interesting Dutch country but the boredom of this was forgotten in the excellent dinner in a former monastery converted into a somewhat synthetic but first class ‘olde Dutch’ roadhouse; the third was a more formal reception by the Town Council of The Hague.

Altogether the conference was both interesting and useful and although the organisation was sometimes rather haphazard those attending were all convinced that their time had been well spent.

Papers available

W. Pirog. The problem of extending the area of action of the Study Committee F.I.D./T.I. and the long range programme of its work.

M. Ducas. The problem of communication between specialist documentation services and users; Terms of reference and basic information on F.I.D./C.R. committee on classification research.

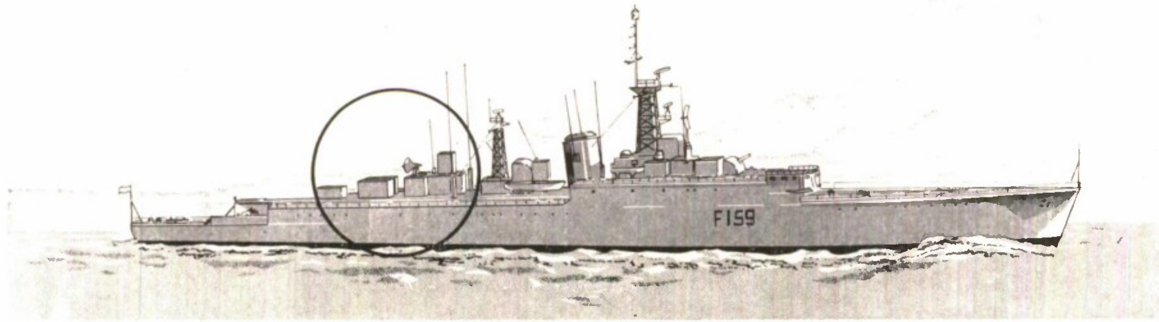
G. Lorphevre, R. Molgard Hansen. Enumeration of classification terms from Vocabularium Bibliothecarii and Vocabularium Documentationis; Committee on classification research activities report for September 1965 to September 1966; Committee on developing countries—Draft programme for 1966-1968; Proposed tasks and scope of the F.I.D. committee on linguistics.

W. N. Locke. Selected bibliography on linguistics and documentation.

N.I.D.E.R. Paper on linguistics—Vocabularium Documentationis; Proposed activities of the committee on the theory of machine techniques and systems; C.C.C. Programme for U.D.C. 1966-68 (Revised) (CC66-36).

H. M. S. WAKEFUL

With Naval Communication Satellite Terminal



Britain's first satellite communication terminal for shipborne use has been built for participation in the Interim Defence Communication Satellite Programme (I.D.C.S.P.). The project is part of the Royal Naval Scientific Service research programme at the Admiralty Surface Weapons Establishment. Initially the terminal has been set up on a land site, but early in 1967 the equipment will be transferred into H.M.S. *Wakeful* to undergo sea trials.

The I.D.C.S.P. is an inter-Service project to test the efficiency of global communications in the military sphere. The " space signment " is provided and financed by the U.S. Department of Defence and managed by the Defence Communications Agency in Washington. The satellites are active repeaters (as opposed to passive reflectors) launched into near-stationary equatorial orbits at a height of approximately 20,000 miles by Titan 3C rockets.

A successful first launch of a group of eight satellites was made in June of 1966. Another launch is planned for early in 1967. Of the eight satellites in orbit seven are available for use in the military communications network. The interim programme will extend to the end of 1967.

The main advantage of the new system over present HF systems is its independence of relay stations. Higher quality of communication should be established and the signals will not suffer from variations in atmospheric conditions and hence reliability will be higher. Although this terminal is comparatively small telegraphy, voice and facsimile traffic can be accommodated.

An agreement concluded between the U.S. Department of Defence and the British Ministry of Defence allows a limited number of earth stations to operate within the system. Three land-based stations with large aerials have been commissioned by the Ministry of Aviation. The first of these is located at Christchurch, Hants.

The provision of a shipborne system presents special problems and the Royal Navy's terminal facility has been built to explore these problems. In particular the terminal is being used to determine:

(a) the communication traffic capacity of a small terminal in a strategic network

(b) the operational procedures necessary to provide an effective mobile communication centre

(c) the techniques for acquiring and maintaining communication in a ship environment of motion and radio interference.

The 6 ft. diameter aerial.

The leader of the project is Dr. Glanville Harries at the Admiralty Surface Weapons Establishment.

The aim has been to develop a system using mainly British equipment and components.

The terminal consists of a 6 ft. diameter aerial, with auto tracking facilities, fed by a high-powered transmitter operating in the military band of microwave frequency. The signal from the satellite is retransmitted at a different frequency. Two separate receivers in the system enables the ship to monitor its own signals as well as those from another sending station.

The transmitter-receiver is being built in the Space Division of Plessey Radar Ltd., at Cowes, Isle of Wight for the initial part of the trials programme. It incorporates a Mullard amplifier for small signal reception. The high power transmissions require water cooling of the transmitter valve and the waveguide systems. The transmitter, receiver, modulator equipment, aerial control and signal processing equipment are contained in two transportable cabins which are designed to be dropped into position on the ship's deck. An auxiliary power supply will be used to augment the ship's power supplies.

A specially designed stabilisation system, using Ferranti gyros, is being installed in the ship by the Admiralty Surface Weapons Establishment.

The trials programme includes an extensive set of experiments with the Ministry of Aviation terminals and a co-operative programme with U.S.N. ships fitted with terminals and operating in the Pacific.

CORRESPONDENCE

To The Editor,
Journal of the Royal Naval Scientific Service
Dear Sir,

I found Mr. Beynon's paper in the September issue, on the casting of copper alloys, very interesting, and have looked up my records of my experience with similar alloys, in the hope that they may be helpful.

In the first place, however, I must express my surprise at the figures given by Mr. Beynon. He gives the compositions and mechanical properties of two batches of castings made by No. 1 Dockyard, another from No. 2 Dockyard, and one melt from No. 3 Dockyard. The specifications for the material are given. In Table 1a, out of eleven melts, Nos. 3, 7, and 9, are out of specification because of tin content, No. 5 is out because of nickel content, and 1 and 7 are out because of tensile strength. Five melts out of eleven are therefore out of specification.

In Table 1b, all the tin contents except No. 2 are outside specification, and the nickel content of No. 2 is too high. None of the six is therefore within specification.

In Table 2, Nos. 2, 3, 5, 6, 7, 8 and 9 have tin contents which are outside specification, Nos. 5, 6 and 7 have the zinc content outside, Nos. 2 and 3 have the lead content outside, and Nos. 9 and 11 have the nickel content outside. Only Nos. 1, 4 and 10 are within specification.

The single melt given in Table 3 has its tin content outside specification.

From 29 melts then, only ten are within specification. This is something which I am quite unable to understand. As long ago as 1934, I was metallurgist and A.I.D. approved inspector in a foundry making aircraft and Admiralty castings, and would have rejected any casting which was not within specification in every respect. These deviations from specification do not justify Mr. Beynon's statement that "— by a process of evolution the present quality standards have been attained which show a marked improvement on those, for example, of 15 years ago."

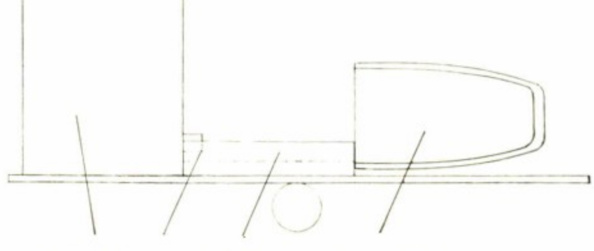
One of the castings which we made was the De Havilland cylinder head, in D.T.D. 174A. (Al 7·5—10·5, Fe 1·5—3·5, Mn 3·5 max, Ni 4·0 max, others 0·3 max, Cu balance). The cooling fins were 3mm thick, and the distance between them was 3mm. This meant that we had to cast at a temperature higher than normal, in order to completely cast the fins. We found a foundry technique which enabled us to make satisfactory castings, and would generally cast about 250 heads per week. All were pressure tested. Any which failed on test were caulked, if it appeared that they could be made gas tight, and were then tested again. If they failed again, they were scrapped.

We had long periods during which there were only a few failures each week. Then we had periods during which the scrap was really heavy, and this was most difficult to account for, as we had standardised every stage of the process, and as far as we could see the bad castings were made in exactly the same way as the good ones.

The moulds were made entirely as cores, and the parts were stuck together, and the whole mould fitted into a box. They were cast by the Durville process, shown very roughly in Fig. 1. It is very useful for jobs where it is necessary to avoid turbulence.

A platform, of suitable size for the casting to be made, is made to swivel on its shorter axis, and can be moved by means of a wheel and gearing. Along the longitudinal axis, a runner is fixed to the platform, semicircular in section, and open at the top. At one end of the runner the mould is fitted, so that the runner feeds directly into the casting cavity. There is a skimmer across the top of the runner at its junction with the mould, which prevents the entry of floating particles, or films of aluminium oxide, from entering the mould.

When the mould has been fixed in position, and sealed to the runner, the platform is turned to the upright position, with the mould at the top, and the crucible, with the exact amount of metal required, at the correct casting temperature, is clamped in position at the bottom, so that when the platform is slowly brought to the horizontal position, the molten metal pours from the crucible, along the runner, and into the mould.



Tilting is continued until the mould is at the bottom, and the crucible is upside down, at the top. The risers should then be full, but not overflowing.

By this method, the metal enters the mould without turbulence, and the metal which enters the mould first goes to the bottom and stays there, the hotter metal occupying a higher place in the mould, and the riser metal coming straight from the crucible. Cooling then takes place from bottom to top, as it should do, because the metal in the bottom of the mould has had the greatest distance to travel, through a cold mould, and has been cooling in the mould for the longest time, when pouring is finished.

In spite of this careful pouring, with strict control of composition and pouring temperature, we had those periods when the scrap was very heavy, and the cause seemed to be independent of foundry technique and metal composition. For instance, after making over 2,000 heads, with only normal "accidental" scrap, we had 65 scrap in one week, out of about 90 made. After trying metal from different sources, with no improvement, it was decided to stop production, and try to find the reason for the failures. Instead of buying hardener alloys, we made up the alloy ourselves, and this appeared to bring about an improvement, but the scrap soon began to mount again.

It appeared that non-metallic inclusions could be the cause of the trouble, and as we had not the facilities for micro-examination, or involved chemical analysis, we decided to try the effect of fluxing, to remove oxides. We used a proprietory flux called "Oxide Remover." To each melt a quarter of a pound of flux was added when pasty, and another quarter of a pound when molten. The whole was then stirred, and the metal poured, the flux being held back. The following results were obtained.

Before using Flux

Total cast	*Good*	*Good after caulking*	*Scrap*	*% good without caulking*	*% good, including those caulked up*
137	66	32	39	48·1	71·6

After using Flux

Total cast	*Good*	*Good after caulking*	*Scrap*	*% good without caulking*	*% good, including those caulked up*
34	28	5	1	82·4	97·0

Some of the heads which had been previously scrapped we then remelted, without the addition of any other metal. They were treated with "Oxide Remover," and again cast. The results were as follows.

Remelted Scrap Heads with No Addition of New Metal, Treated with "Oxide Remover"

Total cast	*Good*	*Good after caulking*	*Scrap*	*% good without caulking*	*% good, including those caulked up*
84	73	9	2	87·0	95·2

We then reverted to the normal method of melting—half new metal and half returned runners and risers. Two days under these conditions gave the following results.

Total cast	*Good*	*Good after caulking*	*Scrap*	*% good without caulking*	*% good, including those caulked up*
38	31	6	1	81·8	97·2

The improvement was quite remarkable, but there was a rather serious disadvantage. This was the way in which the flux attacked the crucibles. They lasted only a third as long as they did before the flux was used, and the attack took place at the surface of the metal, that is where the flux was in contact with the crucible. Thus the crucible was scrapped when perhaps 95% of it was in good condition. It may be that crucibles are now made which would withstand the attack of flux.

Another trouble was the "hardness" of the castings during sawing, and the serious wearing of the saws. One man reported that "bits of nickel" could easily be seen. The risers with the "bits of nickel" were thrown away, and could not be examined, but it would appear that the shiny particles were grains of refractory which the flux had removed from the crucible.

Two ways of reducing the attack were tried. One was to gradually reduce the amount of flux used until a further decrease brought about an increase in scrap, and then to stabilise the amount there. The other way was to try to treat the crucibles with a preparation which would withstand the attack of the flux. I am afraid that I have no records as to the success or otherwise of these measures.

I am somewhat puzzled by the drawings in Mr. Beynon's paper. In the first place, I assume that the lines in the shrinkage key of the left hand casting are intended to be in the opposite direction to those in the other two shrinkage keys.

There are three large risers in the two photographs, and these are labelled "gravity feeder head" on the drawings, but there is a small circle in the middle of the runner, which is labelled "riser." Surely this is the down stick, down which the metal is poured. It certainly could not act as a riser, under any circumstances.

It is surprising to find that even in the accepted casting there are shrinkage areas in the bottom of the casting. This suggests that in spite of the very large feeders shown on No. 3 casting, feeding was not actually taking place in the thin section of the casting. It would appear from the photograph of No. 3 casting that the neck joining the left hand riser to the casting is little if any wider than the casting, and would be surrounded by comparatively cold sand. It would therefore cool as quickly, or possibly more quickly, than the casting itself, and the reservoir of hot metal above it would be quite useless. I am quite aware, of course, that it is easy to criticise other people's foundry technique from a photograph, but not so easy to go and put it right.

While, as Mr. Beynon says, the hope of always getting castings "right first time" will never be realised in the sand foundry, (it will have to be in the die foundry, because of the cost of the die, and the difficulty in altering it), it should be possible, when the prototype has been modified so as to give a satisfactory casting, to get subsequent castings satisfactory, also. The Americans are developing the Premium Quality Casting. This costs much more than the ordinary casting, because of the extra design costs, with probably a large number of carefully placed chills, but the properties are specified not for separately cast test bars, but for parts cut from specified positions in the casting, and for this reason, every casting must be satisfactory as regards composition, physical properties, X-ray appearance, grain size, and surface. There are indications that ultimately Premium Quality Castings will be stronger than composite structures built up from wrought parts, rivetted, or welded together.

E. Carrington, M.Sc., M.I.B.F.
Admiralty Materials Laboratory

Notes and News

Admiralty Surface Weapons Establishment

Admiral of the Fleet Lord Mountbatten of Burma visited the establishment of 19th October to present the Mountbatten Trophy and four Duke of Edinburgh Silver Awards which had been won by members of A.S.W.E. Other recent visitors to the establishment have included Lord Caldecote heading a committee which is examining the workings of the Weapons Department, the Senior Officers' War Course, Rear-Admiral A. M. Lewis (Director General Weapons), Captain C. B. H. Wake-Walker (D.N.S. designate), and Captain M. D. Van Orden (Project Manager and Director—U.S.N. Satellite Communication Project).

The following papers have recently been published by members of the staff:— "Improved Radar Visibility of Small Targets in Sea-clutter," by J. Croney; "A True I.F. Logarithmic Amplifier Using Twin-Gain Stages" by A. Woroncow and J. Croney; "Antenna for Rapid-Scan Decorrelation Radar," by W. D. Delany and R. F. Kyle; *Radio and Electronic Engineer* (Sept. 1966); "Advances in Microwave Direction Finding," by D. G. Kiely. *Proc. I.E.E.*, **113**, 1 (Nov. 1966).

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Bragg Laboratory

The laboratory and the N.O.I.E. were visited by the Lord Mayor of Sheffield, Alderman Lionel Farris, J.P., on 14th October, 1966, when he made a general tour of the Establishment. On 23rd November, 1966, the Establishment was visited by the Mitchell Working Party, which is concerned with the integration of inspection between the three Services. The Party consisted of Mr. W. I. Tupman (Chairman) D.S.20, Mr. E. J. Hiles, D.I.Arm., Mr. C. Burden, D.A.I.S., Mr. H. Hollis, D.C.I., and Capt. J. C. Rome, R.N., C.I.N.O. The laboratory was visited by D.G.W.(N) on 15th December 1966.

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Services Electronics Research Laboratory

Mr. C. H. Gooch attended an International Conference on the Physics of Semiconductors in Tokyo in September 1966, and in the same month, Mr. L. E. S. Mathias paid a visit to the U.S.A. to see establishments and laboratories in Alabama, California, Connecticut, New Jersey and Massachusetts, for discussions on carbon dioxide laser work. At the same time, Dr. S. J. Bass visited Geneva to see the work being carried out on compound semiconductors at the Battelle Institute.

Members of the I.E.E. attending the 6th International Conference on Microwaves and Optical Generation and Amplification in Cambridge, paid a visit to S.E.R.L. on the 14th September.

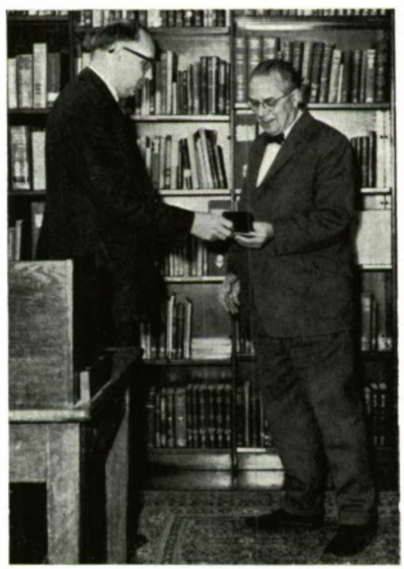
In October Mr. J. W. Allen returned to S.E.R.L. after spending two years at Stanford University, California, whilst Mr. G. P. Wright and Mr. N. H. Rock visited France to attend a meeting and visit establishments in connection with the Anglo-French Working Group on Valves and Semiconductors, and Mr. K. Hambleton, Mr. R. J. Sherwell and Dr. D. J. Oliver visited Munich to attend the Second International Symposium on Microelectronics.

A group from S.E.R.L. attended the 1966 Symposium on Gallium Arsenide held at Reading University on 26th-28th September. The Symposium was organised by the Institute of Physics and Physical Society in co-operation with the Avionics laboratory of the U.S. Air Force. Dr. S. J. Bass and Mr. P. E. Oliver presented a paper entitled "Properties of gallium arsenide crystals produced by liquid encapsulation pulling," and Mr. C. M. Gooch presented a paper "A theory of the high power operation of gallium arsenide lasers." Dr. J. Carroll and Mr. G. Merrett also presented a paper entitled "The use of airbrasion in the production of Gunn diodes."

Professor D. N. Nasledov, Head of the Laboratory of Electronic Semiconductors at the Physio-Technical Institute of the U.S.S.R. visited S.E.R.L. on the 24th November. The visit was arranged by the Department of Education and Science in collaboration with the Foreign Office.

Mr. R. F. Broom left early in October to spend a year at the University of Berne, working on gallium arsenide lasers whilst Dr. F. A. Cunnell has returned to S.E.R.L. after serving two years with C.B.N.S., Washington; Mr. D. Clunie has left to take up this appointment.

The following papers by members of the Staff have recently been published in the outside Press:— "Oscillations Covering 4Gc/s To 31 Fc/s from a Single Gunn Diode." J. E. Carroll, *Electronics Letters*, **2**, 4 (April 1966) 141; "Internal Self-Damage of Gallium Arsenide Lasers." D. P. Cooper, C. H. Gooch and R. J. Sherwell. *I.E.E.E. Journal of Quantum Electronics*, **QE-2**, 9



Mr. E. Stevens of A.R.L. Library being presented by Superintendent A.R.L., on behalf of Her Majesty the Queen, with the Imperial Service Medal in recognition of 27 years meritorious service in U.C.W.E., its predecessors and A.R.L.

(Aug. 1966) 329; "Multi-Velocity Stream Effects and the Efficiency of Slow-Cyclotron Wave Amplifiers." J. E. Carroll. *Electronics International Jnl.* 1st Series, **XX,** 1 (January 1966) 53-64; and "Multi-Velocity Stream Effects and the Efficiency of Slow-Cyclotron Wave Amplifiers." J. E. Carroll. *Electronics International Jnl.* 1st Series, **XX,** 1 (January 1966) 53-64.

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Tenth Nuclear Submarine for the R.N.

The Navy Department of the Ministry of Defence plans to order a sixth nuclear powered Fleet submarine for the Royal Navy early in the New Year.

The announcement was made in the House of Lords by Lord Winterbottom, the Parliamentary Under Secretary of State for Defence for the Royal Navy, who added that the precise rate at which it is intended to continue to build nuclear Fleet submarines has not yet been decided.

Two submarines of this type, H.M.S. *Dreadnought* and H.M.S. *Valiant,* are already in service with the Royal Navy, and two others, *Warspite* and *Churchill,* are under construction. The fifth, as yet unnamed, was ordered three months ago.

This announcement means that the Royal Navy now has a total of ten nuclear submarines in service, building or planned. In addition to the six Fleet submarines there are four Polaris ballistic missile submarines, *Resolution, Renown, Revenge* and *Repulse,* all of which are building. The first of these, H.M.S. *Resolution,* was launched in September, and the second, H.M.S. *Renown,* is to be launched next February.

The new Fleet submarine announced will be similar to *Valiant,* and armed with torpedoes, her primary rôle will be anti-submarine warfare although she will be equally effective against surface ships.

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R.N. World Lead in Gas Turbine Propulsion

The first gas turbine in the world applied to ship propulsion was that developed for the Admiralty and installed in Motor Gunboat 2009 in 1947.

After trials the engine was exhibited at the Engineering and Marine Exhibition at Olympia in September 1949.

Today, 17 years later, the Royal Navy again leads the world with the conversion of H.M.S. *Exmouth,* now taking place at Chatham, to provide the Royal Navy with the first major warship at sea to be propelled entirely by gas turbines.

A six-feet 'flow' diagram of H.M.S. *Exmouth,* which uses one Olympus for full power with two Proteus engines for cruising, was one of the exhibits on the Royal Navy's stand at "The Engineers Day," held at the Science Museum from 18th November 1966 to 14th January 1967.

Other exhibits on the stand included a magnificent model of the Type 82, the Royal Navy's new guided missile destroyer, which like the Tribal and County Class frigates and destroyers, uses combined steam and gas turbine plants.

There was also a full-scale model of the Proteus gas turbine engine, a selection of photographs describing the wide variety of engineering careers available in the Royal Navy, and a Careers Information Bureau where qualified personnel were on duty to answer questions.

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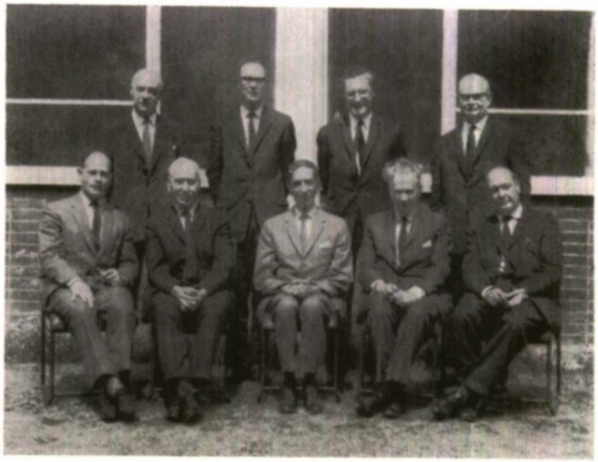
Re-opening of GBR Rugby

The Post Office VLF transmitter at Rugby Radio Station, whose historic call sign, 'G B R' has for forty years provided a vital link with British ships all over the world, was re-opened following extensive modernisation and reconstruction.



Members of the R.N.S.S. Council at S.E.R.L. on 1st July 1966.

Originally designed and built in the early 1920's by engineers of the 'Wireless Section' of the Post Office Engineering Department, the station made history as the world's most powerful transmitter using thermionic valves.

As well as the G B R call-sign, it also broadcast the Greenwich Time Signal at specific times of the day.

At the end of 1965 the very-low-frequency radio transmitter at Rugby was by far the oldest Post Office transmitter in regular use and certainly the most famous. When regular transmission began on January 1st 1926 it broadcast messages in morse on a frequency of 16 *kHz* (*i.e.* a wave length of 18,750 metres).

In wartime, both call-sign and time signal were heard by British vessels in every theatre of operations, from warship to submarine, and were as vital to the morale of our men at sea as the broadcast chimes of Big Ben to the fighting services ashore.

The transmitter had an adventurous history. Its aerial system was extensively damaged in the severe winters of 1940 and 1947 which put it out of action for several weeks on each occasion. In 1943 the transmitter itself was accidentally destroyed by fire, and was rebuilt in six months with only minor changes from its original design.

Reconstruction. More recently it was decided to increase the radiated power and modernize the operating capability of the transmitter, and this has involved complete re-design of the valve amplifiers and the modulating equipment, as well as extensive modification of the main aerial tuning circuit. The old transmitter ceased operations at the end of 1965, after 40 years' notable service, and has been rebuilt by the Post Office Engineering Department in the space of a few months. The most striking changes are in the main amplifier stages, where three amplifier panels each of 18 water-cooled valves have been replaced by three vapour-cooled amplifier valves which can be used singly or in combination, and in the modulator which can generate precision frequency shift signals as well as the original C.W. signals, at speeds up to 72 bands. A new control centre has been installed which will also serve the other two low-frequency transmiters in the building.

The new transmitter began regular use for Navy Department traffic and Time Signal emissions on December 1st.

RETIREMENT

H. J. WELLSTEAD

A recent retirement at A.U.W.E. was that of Mr. H. J. Wellstead, Senior Draughtsman. Harry Wellstead joined the Royal Navy as a Boy Artificer in 1921 and after twenty five years service during which period he served mostly in small ships, he retired with the rank of Warrant Engineer in 1946.

He entered A.S.R.E., as it then was, as a Laboratory Mechanic and three years later passed the Admiralty Draughtsman Examination and was appointed to U.C.W.E., Havant. In 1955 he was promoted to Leading Draughtsman and came to Portland on the formation of A.U.W.E. Promotion to Senior Draughtsman came in 1963 and he retired in early November of last year. The photograph shows Mr. Wellstead being presented with his retirement gift by Mr. S. H. Bowie, the Drawing Officer Manager. The gift which was subscribed to by all his colleagues, is an oil painting of the entrance to the Blue Grotto at Capri.

T. B. BURGE

A recent retirement at A.U.W.E. was that of Mr. T. B. Burge, Senior Scientific Assistant. Mr. Burge joined the Royal Navy at the age of fifteen from the Greenwich Hospital School in 1917. After early promotion to Torpedo Gunner he served in many famous ships, including H.M.S. *Hood*, H.M.S. *Rodney* and H.M.S. *King George V*. He retired from the R.N. with the rank of lieutenant in 1953 and joined A.U.W.E. He was quickly promoted to Senior Scientific Assistant and worked for some years in H.M.S. *Brocklesby* when she was an A.U.W.E. Trials Ship and was closely concerned with the development of Sonars Type 177 and 184. For the past four years he has been the officer in charge of the tender *James Farley*, one of the Establishment's floating laboratories in Portland Harbour.

Mr. Burge has been for the past five years an Independent member of the Weymouth Town Council and is currently serving on the Harbour, Housing and Industrial Committees. He hopes to be able to devote himself fully to these interests now that he has retired.

Mr. I. G. Morgan at his retirement presentation.

I. G. MORGAN

Ivor Goddard Morgan, Senior Experimental Officer, Admiralty Underwater Weapons Establishment retired in November 1966 after 46 years in Admiralty service.

Ivor Morgan entered Portsmouth Dockyard in August 1920 as an Electrical Fitter Apprentice and on completion of his apprenticeship was employed as an Electrical Fitter in the same yard. In 1928 he took the first of the reconstruction examinations for 2nd Class Draughtsmen. Successful in this competition he was appointed in September 1929 to H.M.S. *Osprey*, Portland, at that time the Navy's R. and D. Establishment for Echo Sounding and Asdics. In 1936 he was successful in the open competition for Carpenter Grade Assistants and in 1946 on the formation of the R.N.S.S. he was assimilated as an Experimental Officer and promoted to Senior Experimental Officer in 1950.

When first appointed to Portland he served a short period in the general drawing office and was then seconded to work with Dr. Shuttleworth on the development of the first asdic chemical range recorder. This was a wonderful piece of apparatus, almost a complete factory in itself, taking in rolls of clear dry paper at one end, passing the paper through a wetting plant and after it had been marked by the incoming signal passing it through a drying system to emerge as dried permanent records. After Shuttleworth left Morgan teamed up with the late E. B. D. McKenzie in developing chemical

recorders using pre-wetted paper. This association which continued until McKenzie died in 1943, was responsible for the design of all the Asdic Range and Bearing Recorders and Echo Sounding Depth recorders used by the Royal and Commonwealth Navies during and since the war. Variations on their designs were brought out by the U.S. Navy.

The skill of this team of instrument designers was not confined to chemical recorders. Their talents were used on any problem which could be solved by electro-mechanical means, not the least of these was the asdic attack teacher which could simulate an anti-submarine action over 25 sq. miles of ocean. Many of these teachers were built and placed in various ports and bases around the world to assist in training new officers and ratings and to exercise the skill of escort captains and crews in devising new methods of attacking and sinking enemy submarines.

When McKenzie died Morgan took over the sole responsibility for the design of this type of equipment until with the greater use of electronic devices in the 1950s new techniques were developed. Although in late years he has progressed to the design of equipment for training the large and heavy transducers likely to be used by the Royal Navy in the next 20 years his parting shot is the design and development of a sonar range recorder to be used in the Polaris and other nuclear submarines.

A life-long member of the Church of England Men's Society and warden of his local church, Morgan intends spending his retirement on various church and welfare committees.

The British Acoustical Society

A significant milestone in the history of British Acoustics has been the formation of the British Acoustical Society. This was the result of meetings arranged by the Royal Society between representatives of architecture, engineering, medicine and pure science. A Provisional Council was set-up and the inaugural scientific meeting of the Society was held at Imperial College, London, last May. Appropriately, in view of its importance to large sections of the community, the first meeting was a two-day symposium on aircraft noise and many foreign delegates (as well as British) took part. Subsequently symposia have been held on such diverse subjects as underwater acoustics and acoustical investigations of defects in solids. This breadth of outlook in its lecture programme figures prominently among the objectives of this new society. In this way it is hoped to stimulate interest in all forms of research relating to noise, building acoustics, vibration, hearing and so on.

In order to spread and maintain acoustical interest over as wide an area as possible, meetings are to be arranged at various centres throughout Britain. The Society is also hoping to strengthen its ties with comparable bodies in Europe by holding one joint meeting each year with a national organisation on the Continent. Following this policy, a joint meeting is being held with the Netherlands Acoustical Society next April at Rotterdam.

OBITUARY

J. H. Powell, D.Sc., F.Inst.P.

Dr. James Hugh Powell died on September 28th, 1966, after a short illness. He had retired from the Admiralty in 1954 after 38 years' service devoted to the study of underwater weapons and their effect on ships' structures.

He was educated at Liverpool University and was for a short time on the staff as demonstrator and lecturer in physics. He joined the Admiralty shortly after the outbreak of the 1914-18 war, working first with Rutherford, then with W. H. Bragg on the problem of submarine detection using directional hydrophones. Later he worked on the motion of buoyant mines in tideways, but his real life work began with his association with Dr. A. B. Wood on the recording of the pressures from underwater explosions using piezo-electric gauges. This was an ambitious undertaking in those days, the cathode-ray oscillograph had only just been developed and amplifiers that would deal properly with a 'once-only' steep fronted type of pulse simply did not exist. This meant that all the early work was done with tourmaline gauges that had to be very bulky and heavy in order to give enough voltage and charge to actuate the oscillograph directly. In a sea trial the problems of adequate cable insulation and of positioning such gauges accurately could be very severe. Nevertheless, reliable results were obtained and it was established that the pressure due to a given charge could be predicted to within a few per cent. Also an array of such gauges could be used as a simple sound-ranging device for locating an explosion in depth and distance.

This situation did not really change until the last war, when reliable amplifiers became available permitting the size of gauges to be reduced. Smaller and more robust gauges could then be brought nearer to the charge without damage. Powell played a leading part in directing the development of this subject, and the fact that the British contributions to this field match the results obtained by the U.S.A. with much more manpower is largely due to him. He was a 'founder member' of the Naval Construction Research Establishment at Rosyth. Although not a theoretician, he was fully *au fait* with the theoretical work on the hydrodynamics of the pulse itself and on the damage to ships' structures that was done during the last war, and was always helpful in devising experiments to check difficult points. He made important contributions to the vital problems of how to scale up from model experiments and of how to arrive at numerical measures of 'damage'.

Powell's personality was marked by absolute loyalty and unfailing courtesy to those both below and above him. This was combined with the humility and the willingness to listen to everyone that mark the true researcher. He had great tact, and the imagination to see almost every point of view.

He is survived by a widow, a son and a daughter.

H. N. V. Temperley

THE USEFULNESS OF NON-LINEAR SYSTEM ANALYSIS

J. Lipscombe, R.N.S.S.
Admiralty Underwater Weapons Establishment

SUMMARY

The usefulness of pen and paper analysis of simple non-linear systems is open to doubt in the minds of many engineers.

This is explained by the criticisms in terms that range from "too conservative" to "clearly a need for further education". This paper tries to present the engineers' point of view by means of an example.

Should You Read This Article ?

There must be many control engineers scattered over the world who are perfectly satisfied with computer solutions to their problems, and who have never felt the need for greater insight than that given by linear analysis. There may even be some employed in the R.N.S.S. If you happen to be one of these fortunate people, you have no need to read any further. You already have a full-time job counting your blessings.

Introduction

These days, when practically every control engineer has access to an analogue or digital computer, it is not obvious why pen and paper analysis is necessary at all, even for linear systems. Indeed if computers had been generally available 200 years ago, perhaps the tools of linear analysis would not have been developed by the mathematicians of the day. They would probably have been employed inventing new computer languages.

To consider only linear systems for the moment, the virtue of the pen and paper analysis seems to lie in its ability to generalise. An analogue computer will give a solution to a particular problem. A pen and paper analysis will give not only this, but a feel for the behaviour of a whole class of systems of the type investigated. This means that it will indicate changes in system behaviour for changes in system parameters. This can only be done on a computer by actually changing a parameter, and noting the difference in output. To obtain a general rule from this is not always easy.

Thus we can state that pen and paper analysis of linear systems gives a much broader picture of the system behaviour than does the analogue computer solution. When a control engineer asks for a pen and paper method for non-linear systems, he really wants a technique which will give him an equally broad picture.

For the very complicated systems, it is generally recognised that this is asking for the impossible. All techniques, exact and approximate, linear and non-linear, computer and pen and paper are used to obtain what is usually a limited insight. For example a plant might be linearised to some extent and its parameters lumped before being simulated on an analogue computer. Modern theories on optimisation are then used to formulate adaptive control policies by pen and paper, and these policies programmed into a digital machine which is slaved to the analogue computer. Although a great deal of insight has probably been gained by this procedure, it is limited in the same way as was the computer solution of the linear system that we discussed earlier.

But although we cannot hope for a very general clear cut picture of a complicated system, surely we should be able to do better for a very simple system that is essentially non-linear. Although many techniques have been developed in the past decade, not everyone agrees that we can, and this paper presents a simple example to show why this point of view exists.

One of the first methods to be used for investigating the stability of simple non-linear systems was describing function analysis. This is an extension of linear theory and gives a good indication of the characteristics of certain non-linear systems. However there are many systems which cannot be handled by this method. For this reason other techniques have been evolved. The most publicised of these, and the one that has had the most claims made for it as a very general successor to linear system analysis is a method based on a theorem by Liapunov. It is this that we shall consider in detail.

Liapunov's Direct Method

For the benefit of the non-specialist reader we will briefly outline the principles of Liapunov's direct method. Most modern analytical methods demand that an nth order differential equation be put into the form of n first order equations. Thus if the vector **x** represents all the variables in the control system, and **u** the external quantities used to drive it, then the system is represented by

$$\frac{d\mathbf{x}}{dt} = A\,\mathbf{x} + \mathbf{u}$$

and matrix algebra can be used in its solution. In general A is a function of **x**. The equation

$$\frac{d\mathbf{x}}{dt} = A\,\mathbf{x}$$

determines the autonomous stability of the system.

Several disadvantages and limitations are at once apparent. The sheer labour of manipulating an algebraic matrix of large order precludes pen and paper investigation of large order differential equations. Thus equations which are simple to handle with linear transfer function theory are no longer simple to handle by this method. A second disadvantage is that the main link between the system and its analysis in linear theory, the block diagram, becomes much less obvious. However, in this note we are going to consider a *really* simple problem, and neither of the above limitations will trouble us. We merely wish to find the constraints that must be placed on initial conditions of y and $\frac{dy}{dt}$ for the system described by $\frac{d^2y}{dt^2} + \lambda(y)\frac{dy}{dt} + f(y) = 0$ to tend to a finite solution: in other words be stable.

This system appears to be tailor-made for the Liapunov approach, and if the method is as powerful as its protagonists claim, then we can reasonably expect to obtain deep insight into its stability boundaries.

The above equation is put into the first order form by making $y = x_1$ and $x_2 = \frac{dx_1}{dt}$

Thus the equation becomes

$$\frac{dx_1}{dt} = x_2$$

$$\frac{dx_2}{dt} = -\lambda(x_1)x_2 - f(x_1).$$

(x_1x_2) are called the phase co-ordinates of the system, and the (x_1x_2) plane is called the phase plane.

Now if a positive definite function of (x_1x_2) is formed, v, which surrounds a point on the phase plane with closed contours, this function is called a Liapunov function. This is shown on Fig. 1 for the Liapunov function $x_1^2 + x_2^2 = v$. These happen to be circles on the phase plane.

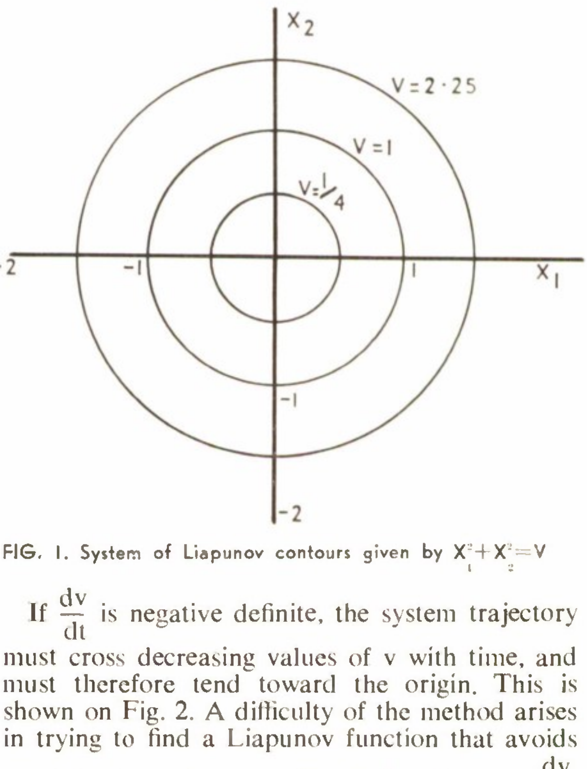


FIG. 1. System of Liapunov contours given by $X_1^2 + X_2^2 = V$

If $\frac{dv}{dt}$ is negative definite, the system trajectory must cross decreasing values of v with time, and must therefore tend toward the origin. This is shown on Fig. 2. A difficulty of the method arises in trying to find a Liapunov function that avoids the behaviour shown in Fig. 3. In this case $\frac{dv}{dt}$ is negative in some regions and positive in others in spite of the general system tendency to the origin. Since the would-be analyst has only the algebra before him, and not the phase plane diagram, he is left little wiser than before. Another drawback of the method is that it is a "sufficient" rather than a "necessary and sufficient" criterion. This means that the revealed extent of stability is a conservative one, but how conservative, it is impossible to determine.

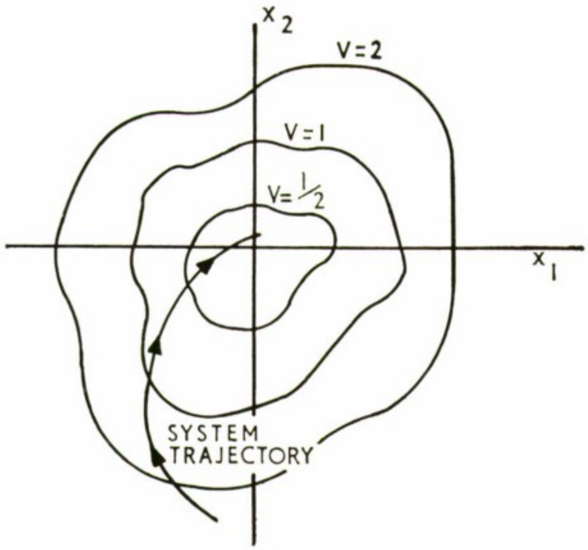


FIG. 2. Liapunov contours and system trajectory

The usual use to which Liapunov's direct method is put is to discover the parameter variations which can occur in a system before it becomes unstable. In this case a Liapunov function, v positive definite is found, and the behaviour of $\frac{dv}{dt}$ is investigated as the parameters are varied. However if the system is stable in some regions of the phose plane, in the sense that its trajectory tends asymptotically toward some point in the region, but not stable in other regions of the plane, then the above procedure breaks down. The reason for this is that while regions of negative definite $\frac{dv}{dt}$ on the phase plane indicate an asymptotic tendency of the trajectory toward a point, there is nothing to prevent the trajectory leaving the region of negative $\frac{dv}{dt}$ and entering one of positive $\frac{dv}{dt}$. The trajectory will then diverge from the point.

However La Salle (1) indicated that the reverse procedure to the one usually adopted holds for systems with stable regions. If $\frac{dv}{dt}$ is made negative definite over the whole phase plane, then if the trajectory enters a closed region of v positive definite it can never leave it, and asymptotic stability is ensured. The stable regions are thus bounded by the largest closed contour of positive definite v.

Stability of the Second Order System

So far we have taken the equation

$$\frac{d^2y}{dt^2} + \lambda(y)\frac{dy}{dt} + f(y) = 0 \qquad \ldots (1)$$

and put it in first order form

$$\frac{dx_1}{dt} = x_2$$

$$\frac{dx_2}{dt} = -\lambda(x_1)x_2 - f(x_1)$$

We have yet to find the Liapunov function v whose derivative $\frac{dv}{dt}$ is negative definite over the whole of the phase plane. There are a few rules which can be used to formulate a Liapunov function, and we are fortunate that one of them, the Variable Gradient method[2] is applicable to our problem. All too often, the analyst is forced to use inspired (or otherwise) guesswork on a trial and error basis to obtain a function.

Before obtaining the function, one further adjustment to the system equation must be made. The origin of the first order form must be moved to make f(o)=0. The reason for this will be obvious later.

No attempt will be made to explain the variable gradient method. Non-specialists may like to skip the next few paragraphs to equation (2).

The variable gradient procedure relies on choosing a simple form for

$$\nabla v \equiv \begin{bmatrix} \frac{\partial v}{\partial x_1} \\ \cdot \\ \cdot \\ \cdot \\ \frac{\partial v}{\partial x_n} \end{bmatrix}$$

and constraining the coefficients to make negative definite

$$\frac{dv}{dt} = \nabla v^1 \frac{d\mathbf{x}}{dt}$$

(prime denotes the transpose).

If the curl equations hold

$$\frac{\partial \nabla v_i}{\partial x_j} = \frac{\partial \nabla v_j}{\partial \mathbf{x}_i} \qquad (\nabla v_i \equiv \partial v/\partial x_i)$$

then the line integral of ∇ v is unique.

i.e.

$$v = \int_0^{\mathbf{x}} \nabla v^1 d\, x$$

Thus the Liapunov function v can be found.

For the system under consideration, let

$$\nabla v = \begin{bmatrix} \alpha_{11}x_1 + \alpha_{12}x_2 \\ \alpha_{21}x + \alpha_{22}x_2 \end{bmatrix}$$

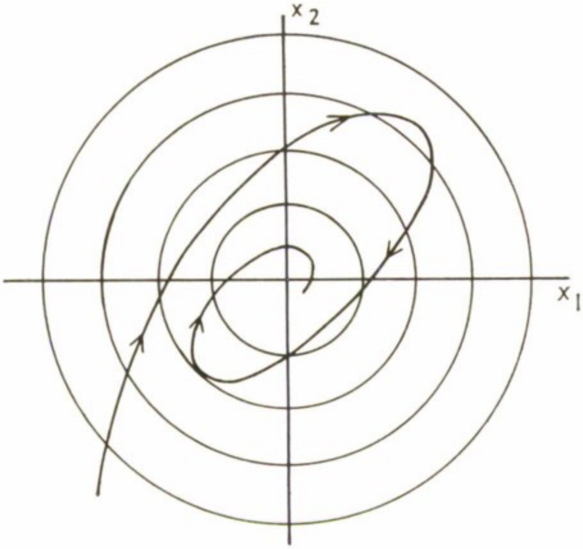


FIG. 3. Stable system giving indeterminate result due to unsuitable choice of Liapunov contours

Now

$$\frac{dv}{dt}=\nabla v^1\frac{d\mathbf{x}}{dt}$$

i.e.

$$\frac{dv}{dt}=x_1x_2[\alpha_{11}-\alpha_{21}\lambda(x_1)]+\alpha_{12}x^2_2$$
$$-\alpha_{22}\lambda(x_1)x^2_2-f(x_1)\alpha_{21}x_1-f(x_1)\alpha_{22}x_2$$

Now because we have made $f(o)\equiv 0$, $f(x_1)$ can be re-written as $x_1g(x_1)$ without $g(o)$ doing anything silly.

Thus

$$\frac{dv}{dt}=x_1x_2[\alpha_{11}-\alpha_{21}\lambda(x_1)-g(x_1)\alpha_{22}]$$
$$-g(x_1)\alpha_{21}x^2_1+[\alpha_{12}-\alpha_{22}\lambda(x_1)]x^2_2$$

To constrain this to be negative definite, let $\alpha_{21}=o$. The curl equations demand that $\alpha_{12}=o$ also.

To eliminate the term in x_1x_2, let $\alpha_{11}=-g(x_1)\alpha_{22}$ If $\alpha_{22}=1$, then $\alpha_{11}=-g(x_1)$

Thus

$$\frac{dv}{dt}=-\quad\lambda(x_1)x^2_2$$

This is negative definite if $\lambda(x_1)$ is positive definite.

Now

$$v=\int_o^{\mathbf{x}}\nabla v^1 dx$$
$$=\int_o^{x_1}f(x_1)dx_1+\int_o^{x_1}x_2dx_2$$

If $f(x_1)$ can be expanded in a Taylor series

$$f(x_1)=f_1x_1+f_2x_1^2+\ .\ .\ .\ f_nx^n_1$$
$$(f_0=o \text{ since } f(o)\equiv o)$$

then

$$v=\tfrac{1}{2}x_2^2+\tfrac{1}{2}f_1x_1^2+(\tfrac{1}{3}f_2x_1^3\ .\ .\ .$$
$$+\frac{1}{n+1}f_nx_1^{n+1})\qquad .\ .\ .\ (2)$$

For a sufficiently small value of x_1 and $f_1>o$, v is a closed negative definite function centred about the origin. This is the Liapunov function that we will use to study the behaviour of equation (1). However notice the restrictions that we have placed on the form of equation (1).

(i) $f_1>0$

(ii) $\lambda(x_1)>0$

This last condition need only apply in the region of the phase plane where v is positive definite.

Example of the Use of the Liapunov Function

Consider the case of a tube discharged torpedo. For the initial part of its subsequent trajectory, various practical limitations prevent active roll control. The problem is to discover the range of tube exit conditions for the torpedo not to roll completely over.

Under very simplified conditions, the roll angle ϕ is given by

$$\frac{d^2\phi}{dt^2}+\lambda\frac{d\phi}{dt}+k\sin\phi=0,\ \lambda,\ k>0.$$

If $x_1=\phi,\ \dfrac{dx_1}{dt}=x_2$

$$\frac{dx_2}{dt}=(-\lambda x_2-k\sin x_1)$$

$$k\sin x_1=kx_1-\frac{kx^3}{3!}+\frac{kx^5}{5!}\ .\ .\ .$$

i.e. $f_1=k,\ f_2=0,\ f_3=-\dfrac{k}{3!}\ .\ .\ .$

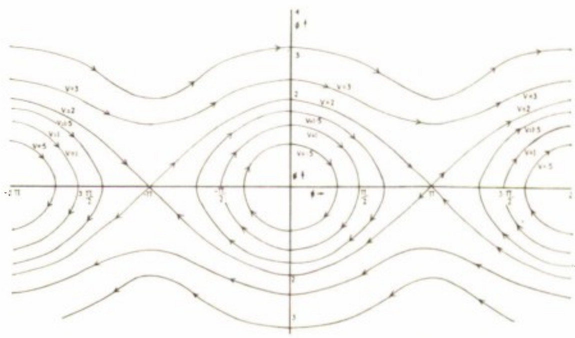


FIG. 4. Liapunov contours on phase plane for $\dfrac{d^2\phi}{dt^2}+\dfrac{d\phi}{dt}+\sin\phi=0$

Thus the Liapunov function becomes

$$v=\frac{1}{2}x_2^2+\frac{k}{2}x_1^2+\left(-\frac{1}{4}\frac{k}{3!}x_1^4+\frac{1}{6}\frac{k}{5!}x_1^6\ldots\right)$$

$$=\frac{1}{2}x_2^2+k(1-\cos x_1).$$

To check that this is the correct Liapunov function, differentiate v.

$$\frac{dv}{dt}=-\lambda x_2^2$$ which is negative definite over the whole phase plane. Contours of v are shown on Fig. 4 for the example k=1.

Although the system is asymptotically stable over the whole of the phase plane (v positive definite, $\frac{dv}{dt}$ negative definite), it is stable to an infinite number of points spaced 2π apart. These correspond to the torpedo not rolling over, rolling over once, rolling over twice, *etc.* However if the initial conditions of the torpedo at tube exit are within the eyelid shaped contour v=2, the negative definiteness of $\frac{dv}{dt}$ will ensure that the phase trajectory never crosses it, and the torpedo will not roll over.

The problem is completely analogous to the large angle behaviour of a simple damped pendulum.

Method Limitations

With this example successfully concluded, we begin to feel confident that the behaviour of any non-linear system of the form of equation (1) will have its stability characteristics as clearly revealed as if it were linear. To check this we will compare the stable area given by the method with the analogue computer result for an equation of the type originally considered by La Salle.

Consider the equation

$$\frac{d^2\theta}{dt^2}+a_1\frac{d\theta}{dt}+a_2\,\theta=k\theta^2. \qquad , \qquad a_1, a_2>0$$

$$\theta=x_1 \qquad \frac{dx_1}{dt}=x_2$$

$$\frac{dx_2}{dt}=-a_1\,x_2-a_2\,x_1+k\,x_1^2$$

$$f_0=O,\ f_1=a_2,\ f_2=-k$$

Thus the Liapunov function becomes

$$v=\tfrac{1}{2}\,x_2^2+\tfrac{1}{2}\,a_2\,x_1^2-\tfrac{1}{3}k\,x_1^3 \quad \ldots (3)$$

(check $\frac{dv}{dt}=-a_1\,x_2^2$ which is negative definite over the whole phase plane).

To find the maximum closed contour of v, consider static stability. For this $a_2\,x_1>k\,x_1^2$

i.e. $x_1<\frac{a_2}{k}$

Thus a closed positive definite contour of v cannot cross the $x_2=0$ axis at $x_1>\frac{a_2}{k}$

The value of v at $(x_1\,x_2)=(\frac{a_2}{k}, 0)$ is given by substitution into equation (3)

$$v-\tfrac{1}{6}\,\frac{a_2^3}{k^2}$$

The area enclosed by the contour

$$\tfrac{1}{2}\,x_2^2+\tfrac{1}{2}a^2-\tfrac{1}{3}\,k\,x_1^3=\tfrac{1}{6}\,\frac{a_2^3}{k^2}$$

contains trajectories which are asymptotically stable to $(x_1, x_2)=(0, 0)$.

The system that we shall solve both analytically and on the analogue computer is one that has a damping factor of about 0·5 at positions very close to the origin. This has the equation

$$\frac{d^2\theta}{dt^2}+6\,\frac{d\theta}{dt}+30\theta=5\theta^2.$$

The maximum closed contour of v is thus $3x_2^2+90\,x_1^2\ -10\,x_1^3=1080$.

This contour together with the analogue computer solution is shown on Fig. 5. The close cross-hatching indicates the area stable by Liapunov, and the wide cross-hatching the actual stable area as found by the computer.

It can be seen that the Liapunov method gives such a gross underestimate of the area of stability that it is virtually useless for design purposes.

Usually in such cases, the analyst cannot find the reason for the bad agreement. He just concludes that he has chosen a bad Liapunov function. This is little comfort, because he has no guarantee that he can find a better one.

In this instance, however, we are fortunate because we can discover exactly why the Liapunov function underestimates the stable region so badly.

Reason for Underestimate of Stable Region

Consider the general second order equation

$$\frac{d^2y}{dt^2}+\lambda(y)\,\frac{dy}{dt}+f(y)=0.$$

The undamped version of this is

$$\frac{d^2y}{dt^2}+f(y)=0$$

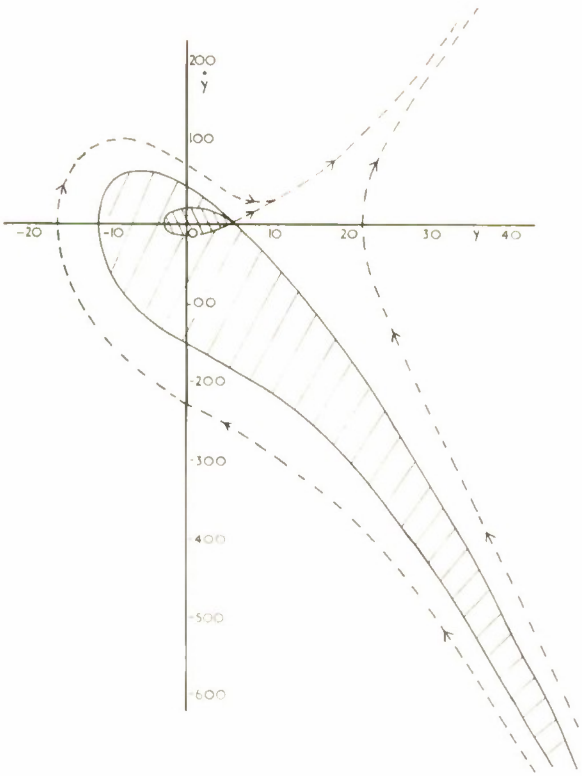


FIG. 5. Phase plane of $\frac{d^2y}{dt^2} + \frac{6dy}{dt} + 30y = 5y^2$

Stable region by Liapunov \ \ \ \ \
Actual stable region / / / / /
Typical unstable trajectories – – –

$$i.e. \quad \frac{\frac{d}{dt}\left(\frac{dy}{dt}\right)^2}{2\frac{dy}{dt}} = -f(y)$$

or

$$\frac{d}{dt}\left(\frac{dy}{dt}\right)^2 = -2\frac{dy}{dt}f(y).$$

Expand f(y) in a Taylor series, and integrate

$$\left(\frac{dy}{dt}\right)^2 = -2\left[f_0y \; \frac{f_1}{2}y^2 + \frac{f_2}{3}y^3 + \ldots\right] + \text{constant}$$

If the constant of integration is put equal to v, and $y = x_1$, $\frac{dy}{dt} = x_2$, then the expression becomes

$$v = \frac{x_2^2}{2} + \frac{1}{2}f_1x_1^2 + \left(\frac{1}{3}f_3x_1^3 \; \ldots \; \frac{1}{n+1}f_n x_1^{n+1}\right)$$

which is identical to equation (2).

Thus the contours of the Liapunov function are the trajectories of the undamped equation. Lightly damped systems would encompass a stable area on the phase plane enclosing and close to the Liapunov area. However systems which are heavily damped could be stable over a considerably greater area. A second order system is rarely designed to have a damping factor in the small of less than 0·5 and Fig. 5 shows the discrepancy which could arise even at this moderate value.

Conclusion

We have shown how the formulation of modern methods of system analysis results in matrix equations. Because of difficulties in manipulating large matrices, the pen and paper analysis which is so successful for linear systems is excluded for anything which is very complicated. The choice is between analogue or digital computation, or a combination of both.

However, the use of pen and paper solutions of very non-linear simple systems appears very attractive. We have investigated the Liapunov stability method applied to a general second order system. The result was that the answer was a gross underestimate of the area of stability. This underestimate was made possible by the "sufficient" not "necessary and sufficient" nature of the method.

For the example we choose, we were fortunate in being able to determine when an underestimate would occur. Usually the analyst is unable to discover this.

The method did not give any greater insight into the characteristics of the system than did the analogue computer solution, and thus as a non-linear equivalent of pen and paper linear methods, it was a failure.

The computer solution shown on Fig. 5 took about one hour to produce, including drawing the patching diagram, patching, and plotting the contours on a co-ordinate plotter. If the time taken to discover the Liapunov function (2) is included, the pen and paper solution took much longer.

It is for reasons such as these that practising control engineers are continuing to doubt the usefulness of many of the new analytical methods that the theoreticians have produced.

References

(1) La Salle, J. P. and Lefschetz, A. Stability by Liapurov's Direct Method with Applications (Book).
(2) Schultz, D. G. and Gibson, J. E. The Variable Gradient Method for Generating Liapunov Functions. *Trans. A.I.E.E.*, Pt. II, **81** (1963) 377-382.

This article is intended to be controversial, and the author will bear stoically the many brickbats which he expects to be aimed at him.

Book Reviews

Teach Yourself Further Calculus and an Introduction to Analysis. By F. L. Westwater. Pp. viii + 212. London; The English Universities Press Ltd., 1966. Price 8s. 6d.

This book is one of the well-known Teach Yourself series, and is intended for those who have already absorbed "Teach Yourself Calculus." The first chapter is an introduction to analysis and such basic concepts as infinity, continuity and limits are considered, these are concepts to which the advanced mathematician must be able to attach precise meanings. The next two chapters are concerned with differentiation, the first being revision, and the second dealing with the differentiation of functions of more than one variable. Integration is dealt with in a similar manner, first a chapter of revision, and then a chapter devoted mainly to definite and multiple integrals.

The student teaching himself should find little difficulty in progressing so far, he may find the next chapter more involved. This chapter is a fairly brief introduction to the somewhat intricate problems of infinite series and integrals. The rest of the book is then comparatively plain sailing, the remaining chapters dealing with a few additional theorems, and introducing differential and partial differential equations.

Two things that are surely required by anyone teaching himself a subject of this nature are a list of books for further study or reference, and a large number of worked and unworked examples. Unfortunately this volume contains neither of these essential requirements, although there are a very limited number of examples at the end of each chapter. If there had been a bibliography one could have recommended this book with fewer reservations; it is an extremely reasonably priced introduction to further calculus. The reproduction of the book is quite good, although there are a few typographical errors which could be confusing to the student who is 'going it alone.'

J. B. Spencer

Physics of Nuclear Reactors. By D. Jakeman. Pp. xii + 356. London; English Universities Press Ltd. 1966. Price 50s.

Dr. Jakeman, formerly lecturer in Reactor Physics at the University of Birmingham, currently at the Atomic Energy Establishment Winfrith, is well qualified by virtue of his experience to make a significant contribution to the existing range of text books on Reactor Physics.

His book, intended for graduate engineers and physicists, is designed to impart an appreciation of the underlaying principles of Nuclear Reactor Physics. While no prior knowledge of Reactor Physics is required of the reader, familiarity with the calculus and with ordinary and partial differential equations is assumed. Some knowledge of elementary Nuclear Physics is also desirable despite an introductory review in the first chapter.

Acknowledging the extensive changes which have taken place in the methods of Reactor Physics in recent years, Dr. Jakeman attempts to achieve a balance between the old and the new in his development of the subject. In view however of the type of reader for whom the book is intended, emphasis is rightly placed on the classical approach which permits a readier understanding of the physical principles involved. In common with most other Reactor Physics text books therefore, Dr. Jakeman's book does in fact rely heavily upon simple one and two group diffusion theory.

Following his brief introduction to Nuclear Physics in Chapter 1, Dr. Jakeman devotes Chapters 2, 3 and 4 to an account of neutron behaviour in non-multiplying media. After a concise development of the fundamentals of neutron energy distributions, a further chapter introduces the Wigner-Wilkins and Heavy Gas models for neutron thermalisation. Although of value this could be omitted on a first reading. A useful, but brief, survey of methods for solving the Boltzman equation precedes a conventional discussion of elementary diffusion theory and age theory, applications of which are illustrated in the derivation of neutron spatial distributions appropriate to point source geometries. The use of group and age diffusion models to evaluate neutron spatial distributions arising from a fast source is briefly introduced.

Neutron behaviour in multiplying media is treated in Chapter 5. For bare and reflected systems, the principles involved in determining critical size and critical flux distributions are clearly presented within the framework of a one group model. The two group approach is then given in some detail using geometries permitting analytical solutions. The treatment of practical configurations and the numerical techniques of multi-group analysis are only briefly introduced—as must be expected in a text of this nature.

In Chapters 6 and 7 the general principles presented in the preceding chapters are developed in application to fast and thermal reactor systems. A fairly extensive coverage of lattice parameters is given, with particular reference to heterogeneous, graphite moderated systems.

Dr. Jakeman's book concludes, somewhat abruptly, with but a single chapter allocated to long and short term reactivity changes, reactor control methods and reactor kinetics. With such a large part of Reactor Physics condensed into a single chapter, the treatment is inevitably rather restricted and contrasts with the detailed development of reactor statics presented in the preceding chapters.

Considering the book as a whole, the standard of treatment is suited to the particular task Dr. Jakeman has assumed. The contents are well organised and coherently presented, probably suiting the physicist rather more than the engineer. It is felt that a more detailed treatment of the material comprising Chapter 9 could have been presented. Despite the predominantly classical approach, the book does introduce the reader to ideas current in nuclear reactor design methods. In this respect the references are of particular value. While they are naturally not comprehensive they do assist the serious student to further his reading by directing him to the recent report literature.

To conclude, the book is easy to read, the figures are well chosen to illustrate the text and the printing and layout are good. At 50s. the book represents good value as a postgraduate introduction to the field of nuclear reactor physics.

D. C. C. Gibbs

An Introduction to Chromatography. By D. Abbott and R. S. Andrews. Pp. vii + 70. London; Longmans Green & Co. Ltd. 1965. Price 7s. 6d.

This paperback is one of a series entitled "Concepts in Chemistry" designed for sixth form and Technical College use. After a short introduction, the simple theory of chromatographic separation is described and this is followed by a more detailed description of paper, thin layer, column and gas chromatography. The final and longest chapter describes simple experiments in the various fields of chromatography. Since considerable space is taken up with line drawings, which are a little elementary and in parts superfluous, a considerable amount of information is packed into the text. The reviewer found that the presentation was too hurried and disjointed in nature, particularly at the beginning of the book. It was necessary to re-read many sentences in order to decide why such a statement was inserted at that particular point. A student, given the book to read, also found this to be so, but found a gradual improvement in the style. A sixth-former, or college student of similar standing, knowing nothing of chromatography, might well get weary of the subject before he reached the more practical part, which is well done. In fact, the experiments seem ideally suited for the intended class of reader. Separation of mixtures of indicators and various coloured inks are described and are excellent ways of demonstrating chromatographic separation. The authors move on to metal ion separation and identification by spraying techniques on both paper and thin layer chromatographs. The section concludes with experiments demonstrating column and gas chromatography.

The section on gas chromatography, both text and experimental parts, leaves a little to be desired, since the simple glassware described bears no resemblance to the complex instruments now widely used in industry and research laboratories; the equipment described in the other sections of the book is similar to that available commercially. No doubt the authors know only too well the type of equipment which can, and cannot, be obtained for schools and colleges, and also the extent of their students' capabilities. On this basis, this book should be useful to the sixth-form or technical college student. The experimental section provides the bonus for the patient reader who has managed to assimilate the text.

A. J .Bloom

Advances in Electronics and Electron Physics, Vol. 21. Edited by L. Martin. Pp. x + 346. New York and London; Academic Press, Inc. 1965. Price $14.

This is the 21st volume in a series of excellent books covering subject matter under the broad heading of its title. This volume contains six detailed contributions on distinct aspects of physical electronics.

The first of these is by P. S. Farago entitled, *The Polarization of Electron Beams and the Measurement of the g-Factor Anomaly of Free Electrons.* A short introduction is followed by a comprehensive description of polarized electron beams which is in turn, followed by a discussion of the effect of macroscopic fields on polarization. The production of polarized beams by two methods, interaction with macroscopic fields and scattering processes, is described in some considerable detail and is followed by a section concerning the removal of polarized electrons from atoms or solids.

The discussions of these principles and phenomena are comprehensive and not excessively mathematical although it is assumed that the reader is reasonably well acquainted with the subject of modern electron physics.

It must be appreciated that the series comprising this and other previous volumes under this title is not a teaching text, but rather is an informative text for workers in the fields of advanced electronics and electron physics.

The remainder of this first contribution describes four methods of measurement of the g-factor anomaly of free electrons in notable detail. Following the paper is a list of some 109 references to papers on the subject matter in this contribution.

The second contribution by C. Snoek and J. Kistemaker is entitled. *Fast Ion Scattering against Metal Surfaces* and after a short introduction, describes the dynamics of two-atom collisions, both elastic and non-elastic types being given attention. This description is somewhat sketchy, based upon mechanics and results quoted from the works of other researchers. The paper is rounded off with a detailed description of five scattering experiments, using solid targets and a list of 45 references.

The third contribution is by David B. Medved and Y. E. Strausser and entitled, *Kinetic Ejection of Electrons from Solids.* The solids referred to in the title, cover metals, insulators and semiconductors. A very comprehensive introduction precedes an elucidatory section concerning experimental techniques followed by a section describing experimental results for both polycrystalline and single-crystal metal, as well as insulator and semiconductor targets.

The theories on this subject are then critically reviewed and followed up by conclusions drawn from the foregoing discussions. A list of no less than 189 references to the work on this subject is included.

C. W. Oatley, W. C. Nixon and R. F. W. Pease describe *Scanning Electron Microscopy* for the fourth contribution to this volume. This is perhaps the most comprehensive, up to date account of electron microscopy and the electron microscope published to date.

The principles are briefly outlined and a short history of the technique is presented. The essential principles of the design and fundamental limitations are then admirably presented in some considerable detail, comprising practical details such as factors affecting contrast, the effects of penetration of incident electrons into the specimen and practical limits of resolution.

Techniques and applications are next given meritous attention, the applications described including, examination of fabrication of integrated circuits, direct observation of chemical changes and examination of biological material and synthetic fibres. Numerous plates admirably illustrate the exceptional performance of modern electron microscopes and some 47 references are included.

The fifth contribution, by L. A. Russell and entitled *High-Speed Magnetic-Core Memory Technology,* is a non-mathematical account of magnetic-core memory arrangements.

The author tries, with a high degree of success, to present the subject such that readers not generally familiar with the subject may find the text understandable whilst those experienced in the field of digital computer design will not find the content too superficial.

Two types of memory are described in detail, the coincident-current or three-dimensional toroidal core storage array and the two-dimensional core memory.

This text is to be recommended to anyone even remotely interested in memory systems and an excellent list of 101 references is included.

The final contribution by V. Ya. Kislov, E. V. Bogdanov and Z. S. Chernov entitled, *Physical Foundations of Plasma Applications for Generation and Amplification of Microwaves* is presented as a specialised treatise of aspects of plasma microwave electronics. The text becomes highly mathematical after a short introduction whilst presenting the subject in a concisely comprehensive manner such that many sections are readily understandable. The subject of microwaves in general belongs to the astronomer and communications engineer and readers within this category will find the content of the paper interesting if the list of 95 references is considered as part of the text.

This volume maintains the standard of the previous volumes in the series and looking to the future, there are some equally interesting papers to be published in future volumes.

D. Robson

Variational Techniques in Electromagnetism. By L. Cairo and T. Kahan. Pp. ix + 150. London; Blackie & Son Ltd. 1965. Price 27s. 6d.

Most mathematical techniques make their first appearance in a limited form as a note in a periodical, or as a paragraph or so in a more lengthy work. Other applications then appear at later dates and a general technique has been born. So often the process stops there and if one wishes to use the method a search through countless references is entailed.

It is therefore especially gratifying to find this monograph, covering a technique and its applications, well written and nicely balanced in content. Indeed the first three chapters summarising some fundamental mathematics, especially Chapter three on Green's function, are well worth reading for their own sake.

Apart from those first three chapters the next is on general principles of variational methods and the remaining eight chapters relating to specific problems are equally divided into four on problems of scattering and diffraction and four on those of cavities and waveguides.

A good bibliography is found at the end of the volume and an appendix on the Dirac notation.

D. P. Valler

Electrical Technician's Work. By R. A. Mee. Pp. 282. Macdonald & Co. (Publishers) Ltd. 1965. Price 40s.

The title of this book is somewhat misleading since the book is in fact a comprehensive collection of model answers to questions that have been set by the City and Guilds of London Institute of Technology in their Papers for Intermediate "Electrical Principles" and Final "Electrical Power Equipment Testing Methods" between 1958 and 1962.

The book is prefaced by a complete index to the worked examples showing the year and question number in the Examination Paper.

Part One of the book deals with problems set in the Intermediate Examinations under the chapter headings: Heat Energy, Mechanics, D.C. Electrical Theory, Electromagnetism, Fundamental Theory, Instruments and Electronics, and occupies approximately one third of the book.

Part two is concerned with questions set in the Final Examinations, and in addition to the topics covered by previous chapters has sections on A.C. and D.C. measuring instruments, measurement of resistance, inductance and capacitance, D.C. generators and motors, A.C. single-phase and three-phase motors, synchronous motors, transformers, switch gear and protective devices, and miscellaneous.

Each chapter is followed by a set of problems, many of which are C. & G. or London University past papers. Answers to these are given at the end of the book.

It is possible that an intelligent student could learn most of the relevant theory by working through all the model answers, but the book should prove most useful to anyone studying for Intermediate and Final City and Guilds Electrical Examinations in conjunction with a recognised course of study and reading of the appropriate text books. It is thought that it would be very useful to electrical students in the Royal Navy, especially those studying by means of a correspondence course, and also to members of the R.N.S.S. who may be studying electrical subjects, or to those who wish to revise their past electrical studies. The author brings in many interesting facets of Electrical Engineering in his Model Answers. The book has but few errors and these are easily apparent. Whenever necessary the model answers are illustrated by good quality drawings.

This book is strongly recommended to students preparing for City and Guilds Electrical Examinations and to others wishing to revise their past electrical studies.

C. F. McCourt

New Application of Modern Magnets. By G. R. Polgreen. Pp. 330. London; Macdonald, 1966. Price 60s.

There is much more in this book than is suggested by the title, because in Part 1, after stressing the importance of the new magnetic materials, it leads the reader through the historical and theoretical background of both permanent and electro-magnetism, and the technology of powders, micropowders and ferrites used in the construction of modern magnets. The chapter on the theoretical background is particularly instructive, and guides the reader from the earliest theories of magnetism to the most modern, explaining the crystal and atomic structure of ferromagnetic materials, with a comprehensive section about the Domain theory. All units are in the M.K.S.A. (S.I.) system being adopted in this country and a very useful table is appended showing the comparison with the C.G.S. equivalent units.

In Part 2, entitled "New Applications," the reader is shown how the new magnetic materials may make Barlow's Wheel of 1822 the best basic principle to use for highly efficient electrical motors. It is in this part of the book that the author demonstrates his great inventive ability, especially in regard to magnetic suspension (levitation) and linear propulsion. It is shown that in this manner one may support heavy loads by the use of ferrite magnets acting in repulsion and thereby have an almost frictionless movement, and to use a homopole linear motor to rapidly accelerate a train for fast and economical transportations of passengers and goods.

Similarly in the chapter on rotating machines the author has demonstrated that by using modern magnetic materials the electro-magnetic field of D.C. motors and generators may be produced by permanent magnets, thereby obtaining the excitation field at the negligible cost of the initial magnetisation. This also brings in an additional safety factor in the use of the motor for braking in electrical vehicles, because the field excitation is always present. In addition, in the case of alternators the increased economy obtained by using permanent magnetic fields is demonstrated, and the possibility of obtaining relatively high frequency electricity from such alternators illustrates the flexibility of this approach.

The book is primarily intended for engineers and students interested in the use of ferrite magnets in a very wide range of applications, and it will be very useful to many members of the R.N.S.S. and in particular to those interested in Fuel Cell research, since the author shows that the use of the newer ferrite materials makes possible the design of D.C. motors working from an input voltage of the order of one volt and correspondingly large currents at high efficiency.

Many ideas which at first sight appear to be revolutionary are shown to be very promising, such as the use of mercury or sodium-potassium alloy for transfer of electrical energy to moving parts with negligible I^2R loss, and the use of varying the mechanical pressure applied to commutator brushes, the pressure varying proportionately to the current.

This is a book which is calculated to make the reader think, and to think hard in places, but having decided to start afresh, with the knowledge that the new magnetic materials make permanent magnets of high intrinsic coercivity and remanence, many of the older ideas which were not practical because of the lack of suitable materials, now become attractive new fields of research and development.

The book contains a useful glossary of the technical terms used, and is liberally illustrated with tables, mostly comparative, graphs and many excellent drawings, but it is a pity that no photographs are included. There are plentiful references at the end of each chapter. The printing and binding are good and the book can be recommended as a stimulating treatise on the subject of permanent and electromagnetism and in particular on the application of the newer magnetic materials.

C. F. McCourt

Worked Examples in Direct Current for Engineering Students. By P. J. Freeman. Pp. vii + 138. London: Macdonald & Co. Ltd. 1965. Price 30s.

This book is intended for students in the first year of Degree, Dip. Tech. and H.N.C. courses, and also for 0·2 of the O.N.C. and T3 of the City and Guilds Electrical Technicians. It is in two parts, the first being a series of questions with answers and the second, the same questions with the solutions worked out immediately below. There are seven sections, starting with simple resistivity and power problems, and leading, via networks, distribution, magnetic and capacitive circuits, to motors and generators.

Since this book is intended for the student, it must be considered from his point of view. Its major advantage over the usual pages of problems distributed by lecturers is the clear way in which the solutions are set out, every step being fully explained. In some cases, alternative methods are given. It is also pleasing to see that the symbols used conform generally to the B.S. specification—a nice point which other writers might bear in mind. (One symbol which seems to have strayed out of the fold is the abbreviation for ampere-turns).

Although the solutions contain most of the relevant formulae, the value of the book would be greatly increased if, before each section, a brief resumé of theorems and formulae could be given.

Lecturers will find the book a mine of good examples to exploit but the student may consider his money better spent on a more formal text book.

J. E. Etheridge

Welding Technology Third Edition. By F. Koenigsberger and J. R. Adair. Pp. viii + 424. London; Macmillan & Co. Ltd., 1965. Price 35s.

This book is a revision of the 1953 edition of Koenigsberger's book of the same name. The book opens with a very brief review of the various welding processes, which are then described in detail in the next 190 pages. The special features of welding ferrous and non-ferrous metals are then discussed for 35 pages and the next 150 pages are devoted to the chemical, electrical and mechanical equipment ancillary to welding. Finally 30 pages are given to various aspects of welding design, draughtsmanship and testing. There is virtually nothing on the metallurgy of welding.

In the preface the aim is stated to be that of appealing to engineers, draughtsmen and shop foremen who are involved in welding, but further it is claimed that consideration has been given to the needs of students on welding courses. One wonders how many present-day students will follow up references such as Trans. Inst. W. January 1938 (with no page number) or even, as on page 252, ETZ 1905. There are no worked examples or examination questions. Readers who are shop foremen will no doubt appreciate seven pages on the design and day-to-day running of generators for acetylene gas, but one page for ultrasonic testing of welds seems rather skimpy. I hope foremen will not take too trustingly the statement on page 50 that low-hydrogen electrodes may be baked at 110° if found to be damp.

The book is well indexed and well cross-referenced, and references to the periodical literature and to manufacturers' handbooks are given at the foot of most pages. Tables from relevant British Standards are also reprinted, with acknowledgements and with the price and address to write to if further copies are required. This is excellent, but it is puzzling to find that in the one book two different addresses are quoted. The illustrations (all except the very first) are clearly reproduced and usefully selected, although in some cases a little dated, and the printing of the text is clear also. About a dozen printer's errors were noted in a length of 400 pages. There were however countless examples of sloppy editing, such as an account of arc phenomena in which on one page we find forces in Newtons and speeds variously quoted in m.p.h. and in cm/sec. In addition we have a formula without units (page 149), photomicrographs without scales (pages 175, 179 and 180), a duplicated reference (page 314), and many cases of ungrammatical or careless writing.

However, this said it must be admitted that at the price the present book deserves to be bought for libraries. Better edited it would deserve more use.

A. P. Bennett

The Scientist. By H. Margenau, D. Bergamini and the Editors of *Life*. Pp. 200. Time-Life International (Nederland) N.V. 1966. Price 35s.

During the last few hundred years science has made many contributions to our way of life. In fact, scientific discoveries of the last few decades now dominate our whole existence. As a result of this, the scientist has become increasingly important until today he is one of the most influential and powerful men in our society. This book has been written to try and give the layman an insight to the scientist's way of life, his problems and the workings of his mind.

The plan of the book is such that colourful picture essays alternate with factual chapters containing a wide

variety of information. It begins by presenting a picture of the world as seen by the scientist; a world full of intricate symmetry and order. The associated illustrations include a mosquito's eye magnified 160 times and an iron crystal magnified 10,000 times. The book then probes into the scientist's home life and tries to find out what makes him tick. Apparently psychologists have discovered that there is a certain combination of traits which characterise the scientific personality. The most striking characteristic is a formidably high level of intelligence (I.Q. approximately 150) but many other interesting facts emerge; for instance he normally regards holidays as an annoying interruption and is usually an agnostic or an atheist. Interwoven with this character essay are some interesting statistics concerning the geographical distribution of scientists in the U.S.A. and also a breakdown in numbers of the scientific community into technical engineers, pure scientists and teachers.

The next section of the book is a picture essay which deals with the development of scientific instruments. Beginning with pictures of astrolabes which were used by early astronomers for determining the positions of heavenly bodies, the book traces the development of the sextant, microscope and telescope. It is brought out how, during the Renaissance, instruments tended to be made only for the wealthy few and a combination of money and craftsmanship made them very elaborate and beautiful. During the 18th century machines were used to make instruments both more accurate and cheaper and once this occurred some of the first real experimentalists like Geurick and Leibig established their laboratories. Sketches of these early laboratories and photographs of some of the more recent buildings and the equipment they contain, form an interesting part of the book.

The scientist has a particular way of solving his problems and this has developed from the branch of philosophy called epistomology. A description of this method is given and it is illustrated by an account of the search for the omega minus particule. This is a nuclear particle whose existence was predicted by a theory in 1962 but it took 114 people two years to detect it.

A particularly interesting picture essay follows which traces the development of the various sub-divisions of science from the seven main branches. The principal divisions are give as Physics, Chemistry, Social Sciences, Mathematics, Astronomy, Earth Sciences and the Life Sciences. A family tree is shown in the form of a very colourful chart for each of these main areas of science and small sketches show the prominant figures behind great discoveries.

The rest of the book then concentrates on some of the more recent aspects of science. It comments upon the communication gulf which has appeared during the last few years due to the enormous growth in scientific knowledge and the increase in specialisation which has accompanied it. Expenditure on science is discussed and is shown to have risen to enormous proportions so that many projects cannot now be financed from a single source. Finally it deals with the impact of science on our everyday life and takes a brief look into the future.

This is a very interesting book and well worth reading. It should appeal to people in all walks of life—especially scientists themselves. The illustrations are very well drawn and most of them are in colour. All the information given adds up to a fascinating account of the development of science and the scientist throughout the ages.

R. G. F. Taylor

The Theory of Practice in Management. By R. W. Revans. Pp. 167. London; Macdonald and Co. Ltd., 1966. Price 25s.

Professor Revans was formerly Director of Education to the National Coal Board and Professor of Industrial Administration at the University of Manchester; he is now a Research Fellow with the European Association of Management Training Centres, working upon the definition of management problems and policies; he is a consultant not only to Universities throughout the world but also to several international firms.

More than happy to possess the book but astonished by his own temerity in agreeing to review it, the reviewer was considerably re-assured by the publisher's blurb—blurb which he assumes must have the distinguished Professor's approval: "There are very few people," it says, "in occupations so solitary that they need no understanding of management at all," and furthermore, it continues, "for the student of management—that is, for everybody who is trying to learn from his own experience—the book might help to establish a few patterns of thought that bring order and meaning out of the confusion and uncertainty of what actually goes on in the world."

With the covers thus opened wide enough to admit the most menial and fears of inadequacy allayed, the book was entered in a balanced state of mind, relatively free from subjective biases. This easy balance, however, did not even survive the introduction, before these short nine pages have been negotiated the reader is aware that he is in communication, albeit remotely, with a rare intelligence.

This book is a collection of six papers presented or published by Professor Revans between 1962 and 1965, together with two papers which are presumably new to the public: in this, as in other respects, this book is similar to the author's companion volume, *Science and the Manager,* published by Macdonald in 1965. The earlier book has already been given an enthusiastic reception in the most influential places and there is no doubt that this volume will merely enhance the Professor's high reputation.

With no intentional disparagement of the influence exerted by the *J.R.N.S.S.*, it is, nevertheless, very gratifying to endorse the opinions of reviewers in places of more eminence: this pleasure is somewhat marred, however, by the difficulty of discovering ecstatic phrases which have not already been lavished on the earlier volume: challenging, provocative, original, revealing, instructive, entertaining, disturbing, refreshing, perceptive, humorous, readable, valuable, these are only a selection of the adjectives already used to describe the author's essays, and these are not words cut out of context and pasted together in the fashion of West End theatres, but really do convey the tenor expressed by Professor Revans' readers.

The author is an educator who has retained the closest possible contact with the people whose practices he wants his teaching to influence. As might be expected in a book on the theory of management practice by a professional educator, the training of managers is always uppermost in the author's mind and five of the eight papers are primarily concerned with education.

The first paper, presented at a New York conference of the Comité International de l'Organisation Scientifique in 1963, is entitled, "Management Education and the University Tradition." The reader is totally disarmed by the first section, sub-headed "The Academic Band-waggon, presented with a devastating analysis of the

deficiencies of existing training methods, regaled with the real needs of management students and finally convinced that only the methods propounded by the author are likely to bring about the desperately necessary transformation in Britain's management training.

The second paper, published in Management International in 1964, lays out a basis for the design of management courses, and two short courses undertaken by the reviewer provided evidence enough that some of the many ideas with which this book abounds have already penetrated some of the peripheral departments of the Civil Service; there is also abundant evidence that the more remote seclusions of that gargantuan structure have remained inviolate to Professor Revan's disturbing influence.

The "Theory of Practice" was published in *Universities Quarterly* in September 1962, and not surprisingly is somewhat anachronistic in this context. It is an appeal for a re-assessment of the traditional academic methods—what is "scholarly, academic and rhetorical" might also be "pedantic, sophistical and doctrinaire". Full of wit and philosophy the paper will never really age, indeed it might be compulsory annual reading for all pedagogues, but its anachronism arises from the fact that Professor Revans has already helped to make his paper out of date; in 1958 he was "like a child trying to build a house of playing cards in a north-easterly gale", his efforts to establish a full time course for practising managers at Manchester College of Science and Technology were frustrated . . . "yet within six years the hurricane had backed". The N.E.D.C., the Robbins Co., the Normanbrook Co., all had worked to such effect that by 1964 five million pounds were released in 18 months to establish the business schools in London and Manchester; "not so much a wind of change, more a forced draught", suggests the author.

The "Nature of Managerial Judgement" is a thesis advocating the use of the scientific method for management problems. "To be told that he must exercise qualities like decisiveness, courage and determination do not help him choose a course of action", nor, one might add, does advocacy of the qualities of technical judgement, leadership, administrative judgement and other such abstractions with which the R.N.S.S. staff may be more familiar.

The "Elements of Organisation" presents statistics associating organisation size and structure, and even the personal characteristics of the top management, with a number of morale indicators. If it were not for the very authentic presentation of the data one might almost suspect that they were invented just to illustrate the author's points.

It is too easy to review a good book and enough has already been said to make it apparent that this book is recommended reading for all, for once a publisher's blurb has the ring of truth, so it is hoped that the titles alone of the last three papers will be sufficient to whet the potential reader's appetite:—"The Manager's Job"; "Our Educational System and the Development of Qualified Personnel"; "The Development of Research into Management and its Problems".

This is a book more likely to please the managed than the managing, exploding too many myths for the latter and providing an abundance of quotations for the former—"Industry is not only a hierarchy of activities for producing goods; it is a way in which large numbers of men live their lives, and what is often overlooked, the only lives they have". Since management education is one of the fashionable activities of the age one might be tempted to dismiss a book coming, as it does, after the wave has broken, but these are the writings of a man who evidently did as much as anyone in this country to create the wave, and essays with the moral and intellectual qualities that these possess should not be demeaned by association with bandwaggons.

Add to a highly perceptive dissection of management activities a commanding erudition, not just the sort of familiarity with great writing which is associated with the *Oxford Book of Quotations* but choice additions to any anthology, and the result is a book which cannot fail to be a pleasure and an education to all but those for whom some the author's many caps may fit—and they, unfortunately, will probably consider their time too valuable to read it.

R. L. Short

Living Control Systems. By L. E. Bayliss. Pp. vi+189. London; English Universities Press Ltd., 1966. Price 25s.

It is a pity that this book is disappointing, as it deals with a topic which is one of the most fascinating and significant in biology. Such a topic should be capable of arousing the interest of even the most prosaic worker in the Physical Sciences, yet the author does not do it justice. The book brings together some aspects of living control systems and servo system theory under the following chapter headings: "Automatic control; The components of living control systems; Elementary properties of servo systems; Responses of servo systems to simple harmonic inputs; Non-linearity in living systems; The performance of non-linear control systems; The properties of some living control systems; Elementary mathematics of servo systems". We are told that the author when writing the book "had in mind that its readers might be, primarily, at the level of the sixth form at schools and the first year at Universities whose training had been either on the mathematico-physical side or on the biological side; and secondly some more advanced students in experimental biology and engineering".

My overall impression of the book is that it is very patchy and unbalanced. The book would have been much improved if part of the servo theory had been omitted and the examples of biological control not only extended in coverage but also developed in depth. For a work entitled "Living Control Systems" there is too much mathematics for the average biologist and too little biology to stimulate the interest of the engineer or chemist. Most of the servo theory is at first year university mathematics level, which few biologists study as part of their course and therefore half of the book will not be read by most biologists—I can only imagine this half being read by specialists in fields of biology where servo theory is directly applicable. The engineer would, I imagine, have all this at his fingertips; if not, he would know where to refresh his memory!

However, my main quarrel is with the biological content of the book. I would think that much of the aim of the book if directed towards interesting the engineer or Physical Scientist in biological problems; yet it is here that I feel the book most falls down. Admittedly, the book is orientated towards those biological control systems which could be interpreted in terms of servo systems, but by restricting its coverage to the given examples I feel that much of the potential value of the book has been lost. Two chapters in the book should be of interest to most readers—"The properties of some living control systems" and "Non-linearity in living systems", particularly the former where the author describes the control of muscular movement, plant orientation, blood circulation, breathing, and size and shape. The last topic, however, is described only very briefly. A much wider range of topics should have been

covered; it is surprising that the whole field of animal behaviour is ignored and the rôle of hormones in controlling biological activities dealt with in a page or so. The hormone complexes governing the activities of insect colonies are so fantastic that I would have thought that no book dealing with living control systems could afford to ignore them—least of all one aimed at capturing the interest of non-biologists. Also, the control of metabolic processses is only briefly discussed, which again is a pity, as this field not only offers a wide range of interesting examples of control mechanisms but also should capture the interest of non-biologists.

Nevertheless, parts of the book are worth reading and I think that no-one could look through the book without finding something of interest. I feel that it fails mainly because it aims at two levels of readers and because it does not do justice to the latest advances and concepts in biology. I am sure that the author is right when he said that "one half of the readers will find some parts of the book familiar and possibly unnecessary and the other half will find the other part to be equally familiar and unnecessary," but I feel that most readers will not gain as much from the unfamiliar part as one would expect from a book on this subject.

D. J. T. Tighe-Ford

The Process of Ageing. By A. Comfort. Pp. 152. London; Weidenfeld and Nicholson, 1965. Price 27s. 6d.

It will be obvious to anyone reading this book why it is predicted that within the next decade the Biological Sciences will dominate research in much the same way as physics did before the war. I defy any intelligent person to read this book and not be stimulated to thought. Dr. Comfort, an English physiologist and a leading worker in the field, gives a comprehensive introduction to gerontology—the study of the biology of ageing. The importance of this subject is shown by the fact that there are over eight hundred teams working in the U.S.A. on problems directly concerned with the control of ageing in man! After reading this book one wonders how this country can be spending so little on research into this subject and when the Establishment will finally realise the implications of the biological sciences.

The book is directed at the intelligent reader in all disciplines; yet, and this is a tribute to the book, it still commands the attention of the research biologist. Development and treatment of the subject are clear and logical, opening very generally with "Old Age, the last enemy," followed by chapters on "Life tables; The Longevity of animals and man; Factors affecting life span—rate of living; Factors affecting life span—radiation; Heredity, sex and longevity; Hormones and senescence; The causes of ageing; The present state of ageing; Ageing in the future."

So many aspects of the subject are discussed that it is possible to mention only a few: from the age of twelve years onwards the human body becomes progressively less vigorous and resistant; various theories about ageing and cell death; do ovaries transplanted from an old to a young animal obtain a new lease of life?; underfed rats have a longer total and reproductive life span than normally fed rats; 800 roentgens of radiation in one dose will kill a man, yet a fruit fly requires a dose of 100,000 roentgens; medicine is not increasing our life span, but only making us live longer; sexual abstinence is harmful to guinea pigs; negative feed-back loops in hormone systems; colloid ageing as exemplified by the difference in texture between veal and beef; why should the fact that "hybrids with dissimiliar genes are notoriously longer lived than inbreds" be a very strong objection to Szilard's stochastic "chromosome-hit" theory of ageing? In this last concept it is interesting to note that Szilard is an atomic physicist and as Comfort remarks, the theory ". . . is a physicist's rather than a biologist's." A physicist might reasonably be expected not to know the relative life spans of homozygous and heterozygous individuals! In the last chapter the author discusses the possibility of prolonging the life span of man and indicates some of the problems that may arise.

Some additional reading is suggested at the end of the book; I would further suggest that an excellent book to read is Professor P. B. Medawar's book on "The uniqueness of the individual," which again is directed at the intelligent layman or scientist, but pitched on a slightly more mentally demanding and philosophical plane. A glossary is provided which appears to be adequate and should certainly be of benefit to most readers, as will be the very detailed index.

The book is written in a very readable style and as an Introduction to Gerontology I do not think it could be bettered. Had the author discussed more details of current research and theory, he would probably have gone beyond the range of the average reader. The only criticism that I would make, however, is that there is no mention of insect juvenile hormones. A hormone which prevents a larval stage from maturing into an adult must have gerontological significance, particularly when it would appear that the action of this hormone may be a repressive one acting on the genetic control of larval cell differentiation into adult characters.

I strongly recommend this book as one that should be read as part of a thinking person's life in a scientific world, particularly by the physical scientist who would like to know something of the expanding rôle of biology. It is reasonable to believe that gerontology will be a major research field in the future, and as the potential results which could arise are so significant, I suggest that no person engaged in administration or research should fail to read this book. It may be a preview of a scientific break-through!

D. J. T. Tighe-Ford

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